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**HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.

JULY—DECEMBER,
1864.**

LONDON:
**SAVILL AND EDWARDS, PRINTERS, CHANDOS STREET,
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THE
HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES:
BEING

A PRACTICAL AND ANALYTICAL DIGEST OF THE CONTENTS OF THE PRINCIPAL
BRITISH AND CONTINENTAL MEDICAL WORKS PUBLISHED
IN THE PRECEDING SIX MONTHS:

TOGETHER WITH A

SERIES OF CRITICAL REPORTS ON THE PROGRESS OF MEDICINE AND
THE COLLATERAL SCIENCES DURING THE SAME PERIOD.

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HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

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PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE.

(A) CONCERNING HYGIÈNE.

ART. 1.—*On the Present State of the Dietary System.*

By Dr. EDWARD SMITH.

(*Medical Circular*, September 21, 1864.)

IN a paper read at the recent meeting of the British Association at Bath, Dr. Smith, after some introductory remarks adverted to the fact that out of 700 poor-law dietaries of paupers in workhouses, not two were alike. The condition of the dietary in the army and navy was until recent years most deplorable. At the latter part of the last and early part of the present century scurvy, due to ill-selected food—biscuits which could not be broken, and salt meat that could not be cut—was rife even in our national navy, and inquiries made by Dr. Budd some twenty years ago, and by Dr. Barnes this year, had shown that this disease was still not infrequent in the fore-cabins of our merchant navy, but it was not found in the master's cabin, nor amongst the well-fed officers, but amongst the ill-fed seamen. The state of the dietary in our prison population was not only less satisfactory than that of our army and navy, but was very unsatisfactory. Dr. Smith referred to several instances in which it had been found requisite to diminish the allowance of food to convicts, and to others in which it had been increased—changes made under the mere influence of public opinion by medical gentlemen on grounds which were illogical, and without making use of the only plan by which the fitness of the change could be proved—that of scientific inquiry into the condition in which the dietary was to be used; and he suggested that a Government officer should be appointed to make the necessary scientific inquiries. With regard to dietary in hospitals, an approach to uniformity was possible, but

at the present there was a great difference. There was the best reason to believe that the dietary of our private schools and colleges and charitable institutions for the maintenance and education of the young had greatly improved, but it was a subject which should demand public attention. There were multitudes of cheap schools in the country in which the sum paid by the pupils was manifestly inadequate to remunerate the proprietors of the schools. A consideration of the dietary of the general community must have regard to two classes at least—viz., the well-fed middle and higher classes and the moderately-fed or ill-fed labouring classes; but time would not permit him to refer to the former further than to offer a few observations on Bantingism. He was not concerned in the medical question as to whether it was desirable to reduce the bulk of a given individual—a circumstance which must always be left to the medical man in charge of the case—but he thought it would be an evil to this nation, both bodily and mentally, if the system of reduction were to become at all general; and that, on the contrary, regarding the whole population, we needed to add to rather than take from the weight of the body. Even in the well-fed classes he had seen very serious diminution of both bodily and mental vigour follow the working out of the plan. There was no necessity for the absolute exclusion of fat and sugar, and the extreme reduction in the quantity of bread, any more than for the great increase in the quantity of meat. The bulk of the body might be reduced by simply lessening the amount of the kinds of food usually taken, and as the reduction was more slowly effected, it was a process attended with less danger to the health. Turning to the question as affecting the labouring classes, he observed that an inquiry of a character unique at least in extent had just been completed by him for the Government, which would afford a clearer insight than had hitherto been obtained into the dietary of our labouring population. It had been made at the houses of the agricultural labourers in every county in England, in Wales, the west and north of Ireland, and the west and south of Scotland, at the houses of certain town populations, silk and cotton weavers, &c., with all the care and minuteness which science could suggest, and the details of which might be found in the Sixth Report of the Medical Officer of the Privy Council. The coarse kinds of bread which were in ordinary use by our forefathers even to the early part of the present century were now rarely eaten. Barley bread was still eaten in the houses of labourers in South Wales, and in the farm-houses of North and South Wales and Anglesea; also in Northumberland and Cornwall and Devon, and in the southern parts of Scotland; but except in certain poor districts in South Wales it was now used as the principal member of the class of breadstuff. Rye bread was nowhere eaten alone. Oatmeal was used by 20 per cent. of the farm-labourers in England. Indian corn was used exclusively in the south of Ireland. Peameal was never eaten alone as a bread, but in the north of Scotland it was sometimes added to oatmeal for that purpose. Rice was used by about 58 per cent. of the population, but never in the place of bread as a breadstuff. The average weekly dietary of farm-labourers was

—for each adult and child $13\frac{1}{4}$ lb. of breadstuff, $6\frac{1}{2}$ oz. of sugar, 5 oz. of fat, $1\frac{1}{4}$ oz. of meat, 52 oz. of milk, 5 oz. of cheese, and $\frac{1}{2}$ oz. of tea; whilst in-door labourers obtained $9\frac{1}{2}$ lb. of breadstuff, 8 oz. of sugar, 5 oz. of fat, $13\frac{1}{2}$ oz. of meat, 18 oz. of milk, and $\frac{3}{4}$ oz. of tea, for each adult weekly. In reference to public institutions he was of opinion that there should be a Government officer of the highest repute upon dietary questions, to whom such questions should be referred by the Government; that in the dietaries of the poor the wives and children are ill-fed, much more so than the husbands; that a large part of the infant mortality and the deaths at puberty from consumption are due to a deficient supply of milk and of other good food, and the almost exclusive use of slops. He also insisted that a man's first duty was to provide sufficient nourishment for his family, even if it were only dry bread, and that until this had been effected nothing should be spent merely to please the taste; and in order to turn the labourer's overtime to profitable account it was desirable that he should have the opportunity of growing plenty of potatoes. Dr. Smith concluded by reference to the present state of the question considered as a science, and pointed out what science had done and what were still the most important defects.

ART. 2.—*On the Food of the Labouring Classes.*

By Dr. EDWARD SMITH.

(*Medical Times and Gazette*, Sept. 24, 1864.)

Dr. Smith divides his subject into two parts, separating the agricultural from the town population, and, further, he subdivides the former class into those fed at the farm, and those who took their meals at home, the former of whom were well fed, and the latter poorly. In Devon, Somerset, Dorset, and Wilts, the breakfast commonly consists of tea-kettle broth, a milk broth or sop, or bread broth (consisting of bread, hot water, salt, pepper, and a little milk, or a little fat of some kind, boiled together), or broth from bacon liquor with condiments, eaten with or followed by bread and treacle, and with or without tea or coffee. Sometimes the children have the broth only, and the wife has tea, bread, and fish, or the husband has bacon, bread, and tea, the wife dry bread and tea, and the children milk, or all have porridge, or the husband alone has it, and the wife and children tea, bread, and dripping; or the husband and children have it, and the wife has tea, bread, and butter; or fried bacon and cabbage is provided; or the husband takes bread and butter only with him; or they all have broth, bread, bacon, and butter; or the husband alone has broth, bread, and a rasher of bacon, or all have tea and bread only; or the husband has bread and cheese, the family dry bread, and the infant sop; or all have bread and cheese. In Northumberland, Cumberland, and Westmoreland, the husband, and sometimes the children, have milk and bread, or milk porridge, or milk and oatmeal pudding, whilst the others have coffee or tea, bread, and butter or cheese. In Herts and Cambridge.

shire the husband has pork, bread, and butter, and tea, or pork, bread, and beer, and the family tea or milk, bread, and butter. In the other counties the breakfast consists of tea or coffee, with bread and butter, the children sometimes having milk and bread, or milk porridge. In a few cases bacon is added to the tea or coffee and bread. The aim is always to provide some kind of meat, with vegetables, for dinner, and in towns, as well as among the farm-labourers in the Midland and Southern counties, this is generally effected daily, whilst in the South-western counties it is not unfrequently restricted to two, three, or four days in the week. This is part dependent upon the income, in part upon the thrift of the family, and in part upon the husband's eating away from home. When meat is not supplied, vegetables, as potatoes or cabbage, are obtained, if possible, and if there be some fat with which to fry the cabbage, or to eat with the potatoes. When this is not practicable, either from the absence of vegetables or fat, boiled rice is sometimes used in the South-west, or a pudding, made generally of only flour, salt, and water, but sometimes with a little suet or other fat added. In the absence of this, the dinner consists of bread and cheese, or of bread and butter. In the Midland and Northern counties it is not unfrequent to provide pudding as well as meat, either daily or several times in the week; and in the South-western counties, broth from bacon liquor and vegetables, with or without suet dumplings boiled in it, is common. It is necessary to remember that there are three, and sometimes four, classes of persons in each family. The husband, who may or may not dine at home, the children, who do not go out to work, the infant who must have some flour food, and the wife. When the husband dines away from home, four circumstances interfere with the plans of the family. 1st. He must have all or the largest share of the special food which he takes, as meat, bacon, or tea, since it is impossible to provide food so that each person may separately have an equal share, when they do not take it together. Moreover, the wife holds him entitled to the largest share of the best food. 2nd. In proportion as the husband has the largest share, the other members of the family have a less share. The children have the dripping from the bacon as their share of the meat, or have bread and dripping or bread and treacle. The infant has sop, with or without milk, and the wife has tea, and the children bread, with, perhaps, a little of the meat or bacon. 3rd. When the husband does not dine at home, it is rare, in the South-western counties, for the family to have any hot food for dinner, except tea. 4th. When there is not a hot dinner, there is almost always a hot supper. In a few instances the dinner meal is the same as the breakfast; but, generally speaking, this is unusual (except with the husband, who has taken his food from home), and it is far more common for it to be like the supper. Amongst the poorest operatives in towns, *e.g.*, needlewomen, and silk weavers, it is customary to buy a little cooked fish, or a little black-pudding, sausage, or bacon, or scraps of meat to be eaten with potatoes or bread. When this is prepared for the supper, the dinner consists of bread and butter or dripping. Hot dinner is nearly universal in wealthy populations. In Wales, the dinner con-

sists of herring or bacon, or meat, with vegetables and broth, or of bread and milk, or gruel, bread and cheese, or of potatoes and butter-milk, with bread and cheese. In the west of Scotland, it is composed of very similar food, as potatoes and fish, or potatoes and milk, or of porridge, milk and bread, or of bread and butter, when away at work, and of potatoes, herring or meat, with broth, on Sundays, or of potatoes, and bacon or meat, and milk and broth, whenever they cook the meat. In the west of Ireland, it consists of stir-about of Indian corn, with a little sugar, or of potatoes and salt, with or without a little butter, and in either case with skimmed milk or butter-milk. Amongst town populations the supper generally consists of bread and cheese and beer, when there has been a hot dinner; but when otherwise, there may be hot meat or bacon, with or without potatoes, and with beer or coffee. The very poor do not usually eat supper, except when so occupied that they could not take tea early, but are restricted to three meals per day. In the country, milk porridge is not unfrequently taken at this meal. Amongst farm labourers in the South-western counties, there is almost always a hot supper, which is quite analogous in kind to the dinner, and is often the only meal, except those on Sundays, at which all the members of the family meet.

ART. 3.—*The Effects upon the System of the Excessive Use of Sugar.*

By M. CHAMPOUILLON.

(*Journ. de Méd. et Chir. Prat.*, Jan., 1864.)

The paper before us was communicated to the Academy of Sciences at Paris. It appears that so far back as the year 1846 the author undertook a series of experiments on himself, in order to supply the Minister of War with information as to the possibility of replacing salt by sugar in the preparation of the preserved meat destined for the use of the army during a campaign.

In accordance with his instructions, M. Champouillon strictly confined himself to the diet which may be accidentally enforced on the garrison of a besieged city by the hardships of war, and for several days in succession lived on the following rations: sixteen ounces of beef preserved in sugar, and four ounces of biscuit; water was his only beverage. Various phenomena supervened in the following order: thirst, sinking at the stomach, distaste for food, nausea, acid regurgitation, epigastric pain, diarrhoea, prostration, and syncope.

"I carefully watched these symptoms," says M. Champouillon, "and the loss of appetite and nausea indubitably proceeded from the absence of variety in my diet; whereas the thirst, heartburn, epigastric pain, and diarrhoea were as clearly referrible to the difficulty of digesting cane-sugar. In proportion to the impression produced by this substance on the organs of taste, it clogs the palate and destroys natural appetite. Thus excessive indulgence in syrups,

sweetmeats, pastes, and highly-sweetened diet-drinks, brings on distaste for food, and annihilates the digestive powers, especially in cases of pulmonary consumption."

After expatiating on the transformation of cane-sugar into glucose, in consequence of its contact with the acids contained in the gastric juice, and on the injury caused by the increased activity imparted to the functions of the stomach by frequent repetition of the process, M. Champouillon showed that in addition to the inflammatory congestion thus occasioned, glucose powerfully contributes to the establishment of a plethoric condition of the system, and that the prevalent opinion that the excessive use of sugar tends to cause pulmonary irritation and a disposition to atrophy, is but too well justified by facts.

In support of this view, the author adduces two interesting cases, one of apoplexy, the other of haemoptysis, in which the agency of this cause was distinctly evident.

"I have often remarked," says he, "in thirty-three years' experience of tubercular disease, that the cough, hectic fever, and night-sweats are increased by the fondness of the patients for sweet substances. I conceive this to be the natural consequence of the combustion of the glucose in the system, a phenomenon which necessarily implies the production of water, carbonic acid, and heat. It is a well-known fact that three and a half ounces of sugar consumed in the human body evolve an amount of heat equivalent to what might be produced by the combustion of thirty-two grains of charcoal. Messrs. Favrot and Silberman have shown that fifteen grains of charcoal are sufficient to impart one degree (cent.) of heat to eight kilogrammes, or sixteen pounds of water. If the capacity of the human body for caloric is the same as that of water, three ounces and a half of sugar will, in a subject weighing seventy-five kilogrammes ($12\frac{1}{2}$ stone) raise during their combustion the temperature of the body four degrees and a half (centigr.)."

The practical conclusion of this paper is that it is desirable to reduce within as narrow limits as possible the consumption of sugar, especially in cases of tuberculosis, and to replace that substance by honey, or a decoction of liquorice.

ART. 4.—*On the Relation and Special Application of Fat and Sugar as Respiratory Fuel.*

By DR. THOMAS HAYDEN.

(*British Medical Journal*, April 15, 1864.)

In one of the papers read at the recent meeting of the British Association for the Advancement of Science, Dr. Hayden stated his belief that fat and sugar possess different values as food; that they undergo different transformations, and, during these transformations, subserve distinct purposes of economy; that the period of their retention in the body is the same; that they are not mutually con-

vertible; but that ultimately they pass out of the body under the common form of carbonic acid and water, and are jointly concerned in the production of animal heat. After noting the mechanical purposes of adipose tissue, the author alluded to the provision made in the fat deposited in the body, for the maintenance of animal heat during a certain period, under circumstances of total deprivation of heat-producing food. But before deposited fat can become available for this purpose, it must undergo disintegration, or disassimilation, and be reabsorbed into the blood. Dr. Hayden did not deny that the adipose, like other tissues of the body, is constantly undergoing gradual molecular destruction; but what he desired to convey was that fat must be always in the first instance assimilated, and can, under no circumstances, be applied to the maintenance of animal heat before undergoing the twofold process of constructive and destructive assimilation. Amylo-saccharine substances, on the other hand, are immediately and directly passed off from the blood, and are never assimilated in the proper acceptation of the term. Of the starch taken in as food, however, a certain proportion escapes the action of the saliva, and is deposited in the liver, probably in the hepatic cells, whence it is drawn to supply heat when the amount of sugar in the food recently taken happens to be insufficient, or when the body is exposed to highly refrigerating influences. Having referred to the experiments of Pavy, Bernard, Sanson, Poggiale, and Roguet, Dr. Hayden said that he was engaged in a series of experiments, and, from the observations he had so far made, had arrived at the following conclusions:—The amount of fat deposited in the body is regulated by the absolute and relative quantity of oleaginous and saccharine matter in the food taken; both substances, taken in a large quantity, cause excessive deposits of fat. If the fat taken be insufficient, even though the sugar be in excess, no increase in the deposit of fat takes place, but rather a decrease; obviously in consequence of ordinary molecular absorption, to which the adipose, in common with other tissues, is subject, not being counterbalanced by assimilation. If the fat taken be in excess, whilst the sugar is insufficient to meet the immediate wants of the respiratory function, still the deposit of fat may not undergo increase, but the contrary; apparently because a portion of that already deposited must undergo reabsorption into the blood for the purpose of supplying heat. Fat is, therefore, as a heat-producing substance, only supplemental of sugar, which is the ordinary *pabulum* of respiration. Saliva, like gastric juice, is secreted in quantity strictly proportioned to the immediate wants of the system, and quite irrespectively of the absolute quantity of food taken; a certain proportion of the starch of the food, varying according to the quantity taken, and the necessity of respiration, escapes the converting action of the saliva, and is stored up in the liver. This liver-starch is being taken constantly back into the blood to supplement the respiratory elements of the food, and in the blood is converted into sugar, probably next into lactic acid, and finally into carbonic acid. Hence the presence of sugar, normally, in small proportion in the blood of the right side.

of the heart; hence, likewise, its presence in the right heart of animals fed exclusively upon meat, in whose portal blood not a trace of sugar is discoverable.

ART. 5.—*On the Possibility of arresting the Development of Epidemic Disease by the Use of Anti-Zymotic Agents.*

By DR. ROBINSON.

(*Medical Times and Gazette*, Sept. 26, 1863.)

Dr. Robinson read a paper on this subject before one of the sections of the British Association for the Advancement of Science, at its meeting at Newcastle.

The author commenced by referring to the circumstance of the analogy between many of the phenomena of fevers and other zymotic diseases and the ordinary process of fermentation having been perceived and recognised by Hippocrates and the oldest writers on medicine. Their idea was that a poisonous ferment, existing in the atmosphere, entered the mass of blood and induced in it a series of changes, which gave rise to the excessive heat and other peculiarities of that class of disease. At the present time, this doctrine, modified by the discoveries of Liebig and other chemists, has been adopted by most physicians, and forms the basis of the classification of disease framed by Dr. Farr, and used by the Registrar-General. It thus supposes living germs to exist in the atmosphere, which, when introduced into the body, give rise to a specific and regular series of morbid actions, pursuing a definite course in a definite time, as in smallpox—those germs being disclosed and multiplied, and producing others capable of reproducing in other bodies the same succession of changes—other lethologists have supposed that the atmospheric poison acts on the blood chemically, by giving rise to what may be termed catalytic actions—while the author is disposed to believe, from what he saw during the cholera epidemic in Newcastle in 1853, that some of these volatile organic matters in the atmosphere are capable of acting on the human body as direct poisons, and that this inanimate volatile poisonous matter also furnishes nutrition to the organic germs suspended in the air. After these preliminary remarks he proceeded to refer briefly to a number of scattered facts, which seemed to him to indicate the existence of a great principle, which might hereafter be found applicable to the prevention or mitigation of epidemic diseases by the direct use of substances capable of arresting the process of morbific fermentation. He mentioned the following facts as converging to this conclusion : 1. Antiseptic substances, ranging from simple innocuous matters, such as sugar, up to the powerful metallic poisons, such as corrosive sublimate, and forming a very numerous and diversified group, have long been known to be capable of arresting the putrefaction of animal and vegetable structures. 2. The same substances prevent the formation of fungi, as is seen in the use of solutions of metallic

salts in taxidermy, in the prevention of dry rot, &c. 3. Many of those agents are also known to arrest at once the process of fermentation, as, for instance, sulphurous acid; and Emi and other chemists have observed under the microscope the rapid stoppage of the vitality of the yeast-plant when a solution of arsenious acid was added to the fermenting liquor. 4. The formation of the fungus in and on the plant, which causes the vine disease, is prevented by applying sulphur to the affected vines. 5. In Cornwall it is believed that the arsenical fumes from the tin-calcing furnaces exercised an influence over the potato-plants in the neighbourhood, which preserved them from the disease then affecting other parts of the same county. 6. It has been found that when a species of fermentation has taken place in the human stomach resulting in the development in large quantities of a minute organism (the sarcina ventriculi), this morbid action can be controlled and stopped by the direct anti-zymotic influence of certain salts, such as sulphate of soda, in doses perfectly compatible with the patient's safety. 7. In different parts of the world, among different races, a belief has long existed that certain antiseptic substances, of which arsenic may be taken as the type, are capable of acting as antidotes or preservative and curative agencies against atmospheric and other poisons, and in some cases that popular belief has proved to be well-founded. The experience of the multitude discovered the value of arsenic as a cure for ague long before it was recognised as such by physicians. The arsenical fumes of certain works in Cornwall were stated by the late Dr. Paris to have stopped the ague previously endemic there. More recently it has been stated that the arsenic eaters of Styria are peculiarly exempt from fevers and other epidemic diseases. And in India the natives have long used arsenic as an antidote for the poison of snakes. Dr. Robinson concluded by expressing a belief that these scattered observations were not only sufficient to justify and necessitate further inquiries in this direction, but seemed in themselves to shadow forth the outline of a great law which might at some future time be productive of immense benefit to mankind.

ART. 6.—*On the Sources of Entozoa.*

By Dr. T. S. COBBOLD.

(*Medical Circular*, Sept. 28, 1864.)

At the meeting of the British Association recently held in Bath, Dr. Cobbold read a paper "On Meat as a Source of Entozoa." Referring first to beef and veal, he described the various species of tape-worm to be found in cattle, and spoke particularly of one, of which a single larva was capable of supplying 45,000 eggs, enough to poison the whole of Bath. He also referred to the difficulty of detecting these parasites, even by a skilled veterinary surgeon, but added that all danger of injury to mankind from their presence, was averted by cooking the meat at a high temperature—say 212 Fahrenheit. Speaking of mutton, he said that most of the intestinal

worms inhabiting sheep did not appear capable of living in the human body ; still it was a wise precaution never to eat meat underdone. Pork was the most injurious, and there was now no question that the trichina inhabiting the pig was communicated by the pork eaten, or that a considerable proportion of the deaths assigned to epilepsy were due to the tape-worm embryo set free by the poor pork-consuming population. If the poor would only abandon their semi-civilized habits of eating raw or half-cooked meat, the evil would soon cease altogether. A great variety of entozoa were to be found in game, but they were, for the most part, of innocuous species. In fish they were more abundant than in either birds or mammals, but there was reason to believe that fish might be eaten either cooked or raw without danger to the consumer.

Dr. T. S. Cobbold also submitted papers upon vegetables and fruits, and upon water, as sources of entozoa. In the former paper he had spoken of the *tænia* in its mature state, and he now proposed to speak of its introduction into the body in larval condition. There was no doubt that entozoa were introduced with vegetable food. Small molluscs harboured larval parasites in prodigious quantities, and they were the source of one or more of the parasites that occasionally invaded the human form. These entozoa might be taken in drinking water, but they were much more likely to be taken from water-cress, or other vegetables of the kind. It was necessary with all vegetables that the greatest cleanliness should be observed in preparing them for the table, and care should be taken to avoid swallowing these small molluscs, which were very likely to escape observation. A large species of the tape-worm, discovered in Egypt, would, he was afraid, be brought to this country at some time from our colonies, and if ever it got place amongst us it would be difficult of extermination. Eggs and living specimens had been found in this country, both in men and monkeys, but only to a very small extent. He was the first to discover it in the monkey. As to the little thread-worm, he had never been able to rear it on apples and pears, and there was no evidence to show that any species of entozoa was derivable from fruit. It was not likely that fruit was ever an intermediate habitation for any of the parasites which ordinarily occupied the human body. A great many evils in children were charged to eating unripe fruit, but, as far as entozoa were concerned, that fear was entirely groundless ; and if they should be so introduced, the chances were that the larvae would be taken from the surface of the fruit. With regard to celery, cabbages, and all the ordinary market-garden vegetables, he might say that all decomposing animal and vegetable matter maintained entozoa, and the more filthy the water or liquid manure employed to secure the fertility of the garden, the more likely was a supply of entozoa to be taken with the vegetables grown upon the land. The most careful washing was, therefore, required, and it had been suggested that vegetables should also be soaked in salt. Turning, then, to water as a source of entozoa, the first species he would draw attention to was the ancient guinea-worm, which was supposed to be the fiery serpent of Moses. Parasitic larvae might be found in

water that was to all appearance perfectly pure; but speaking generally, it might be inferred that fresh spring water was perfectly innocuous. The same thing could not be said of water stored in large tanks in hot climates. The people of Great Britain suffered from entozoa which must have been introduced by drink in some form or other. Amongst this class the smallest was one-tenth of an inch long; it carried 30,000 eggs, and went through marvellous transformations. The presence or absence of the larvae of human entozoa in water was dependent upon the place from which the supply came, and upon the condition of the water. The pork measles might be readily communicated to human beings in this way; and there was another species taken from water, the habit of which was to ensconce itself in the brain, causing death, which the Registrar-General invariably set down as due to cerebral disease. The way in which it reached the brain was from the coats of the stomach, through the circulating medium. There was one kind inhabiting dogs which was often communicated to the human being. One-sixth of all persons who died in Iceland perished from a little creature so small that, in its larval state, it could scarcely be seen. If neither dog nor wolf existed we should get rid of these species altogether. No one need drink water impregnated with these entozoa. Water to which dogs had no access could not contain them; neither were they likely to be found in spring or well water. Open waters, into which the carcasses of dogs were occasionally thrown, would probably contain them, and the eggs might be carried to food washed in such water. The danger would be got rid of if the water was always carefully boiled, filtered, or distilled; but a filter to be effectual ought not to pass anything larger than the one-thousandth of an inch. Sand and charcoal filters were of very little use. Paper filters should be employed. All entozoa not preserved for scientific experiments should be destroyed by fire, and under no circumstances should they be thrown aside as harmless refuse: and he would press upon butchers, knackers, and others, not to throw doubtful offal to dogs frequenting their neighbourhoods. Then as to beer, porter, &c. All he need say with regard to these fermented drinks was, that he believed them perfectly harmless. Even though impure waters should have been employed, the boiling of the wort would be alone sufficient to destroy any number of parasites. Unfortunately, in the case of unfermented drinks, such as ginger-beer, cider, and the like, there must be some uncertainty. All must depend upon the source and the supply of water. They might, however, conclude that the manufacturer got his supply from the purest source open to him, and that, therefore, the public need be under little or no apprehension. In regard to wines, the same remarks were applicable. Alcohol added to water was sufficient to destroy the parasitical eggs, but he questioned whether the amount of spirit in our home-made wines was sufficient for the purpose.

ART. 7.—*On the Use of the Beard.*

By Dr. BELCHER.

(Dublin Quarterly Journal of Medical Science, Feb. 1864.)

In a long and learned paper on the hygienic aspect of pogonotrophy, we find the following suggestive remarks on this subject:—

"It is assumed that the beard was intended for use in the animal economy. We ask, for what use? Looking at the physiognomy of the question we find that it gives character to the face, and makes a countenance which would without it appear weak, appear full of reflection, force, and decision. It serves to conceal the thoughts, hiding the most expressive features in the face, and saving the man from those betrayals which would pull down his dignity and render him often an unequal combatant in the competitive struggle of every-day life. At the same time, the general absence of bearded men makes us unfamiliar with those peculiar shades of dignified expression only to be found in the bearded face, and well known, for that reason, as well as for its artistic beauty, to the great masters of painting. The bearded face was thus a favourite study with Tintoretto, Titian, Rubens, Vandyck, and Rembrandt, who all wore beards themselves.

"A woman is frequently betrayed by the quivering motions of the lips. The great tragic parts are played by shaven actors, who know well that concealing the thoughts would not answer their purpose; and large bets are made at prize-fights in England because of the presence or absence of resolute determination in the shaven faces of the pugilists. The minute and disgusting report of the *Times* newspaper (Dec. 11th, 1863), while pandering to the degraded savagery of a large class in England, establishes beyond question the point just referred to, in the account of the recent disgraceful prize-fight at Wadhurst.

"We are told, on the highest authority, that woman's hair was given her 'for a covering'; and the same applies, in a modified degree, to the beard in man.

"The compensating power of nature may be seen in what is understood to be a well-ascertained fact, that, in another form—the beard—man is provided with a substitute for the profuse 'covering' of woman. The scanty hairs on the head are counterbalanced by the beard; the profusion of the latter is in proportion to the tenuity of the former. Women, beardless men, and children will be found to have longer and more luxuriant hair on their heads than bearded men. But, it will be asked, why this difference? Do not women and children require the same protecting covering as men? By no means. Women and children were intended to live chiefly in houses, secure from the constant exposure to which man must necessarily be subject while engaged in the primitive, divinely-instituted, and, therefore, most natural and healthy, employments of the agriculturist and the shepherd.

"The great fount and origin of nervous sensation and action, and

several of its important offshoots, were intended to be covered with hair. The sensitive division of the fifth pair of nerves, supplying the principal part of the face, and its motor division, supplying the muscles of mastication, illustrate this fact ; and also the *portio dura* of the seventh pair, which supplies motion to all the muscles of the face except those of mastication. The superior laryngeal nerve, which, among other distributions, goes to the epiglottis and the membrane lining the larynx ; the inferior laryngeal nerve, which is distributed to most of the proper laryngeal muscles, and is believed, by the majority of modern physiologists, to be the true nerve of voice, will certainly not be injured in their functions by the natural hairy covering of the parts to which they are distributed. It may be further assumed that the protection of the teeth, the uvula, the larynx, the epiglottis, and the tonsils is furthered by this wise natural covering. The protection of the larynx is of the last importance, when we consider the extensive range of sympathetic connexions maintained between that organ and the great vital functions of digestion, respiration, and circulation, ‘through the medium of the laryngeal, cardiac, pulmonary, cesophageal, and gastric branches of the eighth pair ; all of which are still further associated, not only with one another, but also with the great nervous centres, by their common and frequent communications with the great sympathetic or ganglionic system in the neck, in the chest, and in the abdomen.’

“One part of the beard, the moustache, is said, by the very general experience of wearers of it, to act, to a large extent, when unclipped, as a respirator of the best kind. It has the credit of absorbing the moisture and miasma of fogs, and of straining out the dust and soot of large towns : of taking heat from the expired breath and supplying it to the inspired cold air. The beard undoubtedly acts as a comforter. Moffatt and Livingstone, and other travellers, affirm that no wrap can equal it at night ; and we all know how the fox-hunters of this present day are in the habit of tying both the fashionable donkey-eared whiskers together, to protect the throat when riding against a piercing wind. Not only does the beard protect against cold, but against heat also, like the thatch of an ice-house or of a bombproof Indian bungalow. Perspiration moistens it, and evaporation cools the surface. With this natural protection, a man may go from a hot room into a cold one, or from a pure into a noisome atmosphere, with very much less danger than his beardless associate.”

ART. 8.—*On the Inoculation of Oidium in Man.*

By M. MÉLIER.

(*Journal de Méd. et Chir. Prat.*, Mai, 1863.)

The three cases following, forwarded by Dr. Collin, Inspector of the Thermal Springs at St. Honoré, are the facts upon which M. Mélier bases his paper. The paper itself was brought before the Academy of Medicine in Paris.

CASE 1.—A man, suffering from paroxysms of fever returning at night, with a hot skin and a dry tongue, declared that in dressing his vines he had inflicted on his leg a slight wound, which gave him much pain. The wound, trifling in aspect, was, however, surrounded with an areola of inflammation. In the course of three days, the entire leg swelled enormously, and was covered with large, dark-coloured blisters, filled with serum. Deep incisions were performed, and the following symptoms have since appeared in succession ; stupor, eruption all over the body, resembling scarlatina, *thrush* ; diffused phlegmon of the leg and thigh. Mortification of all the parts situated below the phlyctæna has now set in ; the gangrene makes no further progress, but the patient's life is still in imminent danger.

CASE 2.—The parish priest of the same village, also suffering from an attack of remittent fever, bore in the left hand a scratch inflicted twenty-four hours before opposite the first joint of the little finger. The next morning, dark phlyctænae appeared on the back of the hand, and gangrene of the fingers set in. M. Collin was then informed that the wound had been inflicted while the reverend gentleman was *cutting a vine-stock*. Surmising that the oïdium might possibly have something to do with the symptoms, he repaired to the vineyard, and found the plants covered with the parasite. The symptoms observed in the first case have all recurred in the second ; fever, stupor, exanthema, *thrush*, diffused phlegmon of the fore-arm and arm ; the mortification has rapidly extended, and its progress is not yet checked.

CASE 3.—About three weeks before, Mr. D., a dispensing chemist, slightly abraded the middle finger of the right hand while dressing a vine covered with the fungi of oïdium ; the finger became promptly the seat of blisters, and the parts beneath mortified. Deep scars remain, and the inflammation, which fortunately did not assume a phlegmonous character, extended to the fore-arm and arm.

At a subsequent meeting of the Academy, M. Mélier stated that one of the patients had died, and that another was still in a very precarious state. M. Collin, said he, writes that three other cases have occurred under similar circumstances. The treatment adopted consisted in the exhibition of bark, tonics, and stimulants.

ART. 9.—*On the Arsenic-Eaters of Styria.*

By Dr. CRAIG MACLAGAN.

(*Edinburgh Medical Journal*, September, 1864.)

After some introductory remarks, Dr. MacLagan proceeds :—

“Dr. Macher, now resident at Stainz, who is thoroughly acquainted with all the medical matters of Styria, and whose experience during a long period of active professional work there makes his opinion of no small value, informed me, that, although cognizant of the existence of the practice, he had little personal experience in the matter. He related to me, however, one case, in which a woman, who had been tried for poisoning her husband with arsenic, had been acquitted from ‘want of evidence,’ the plea for the defence being that the man had been an arsenic-eater. Though this defence was generally believed to be false, it shows, at

all events, that the practice has in a court of justice been admitted to exist, and has served, in at least one instance, as a successful ground of defence.

"Dr. Knappe, of Liegist, in Middle Styria, an hour's journey from Gratz, was my next informant; and when I first spoke with him of the case of J. W., already alluded to as the subject of experiment, and whose urine had been examined by Dr. Schäfer, he described him as a small, strongly-built man, with a great muscular development, a woodcutter by trade, who had taken the drug for a period of twelve years. Dr. Knappe further stated to me that while personally ignorant of the actual existence of an arsenic-eater in the neighbourhood, he could go with me to Upper Styria and show me the above man, but he suggested that we should first make inquiry in the village whether or not any of those persons described to him as indulging in the habit could not be got to take a portion of it before me. I accordingly slept in Liegist that night, and next morning I had the satisfaction, in presence of Dr. Knappe and my companion Dr. Rutter, of having my first interview with an arsenic-eater.

"CASE 1.—Mathias Schober, a healthy-looking, fresh-complexioned, fairly muscular young man of the age of twenty-six years, and about five feet nine inches in height, a native of Liegist, and employed as a house-servant there, said he had taken hütterreich for about a year and a half, not, however, white arsenic, but the yellow arsenic or orpiment, of which he took a specimen from his pocket and showed it me. Of this I retained a piece for chemical investigation. He informed me that he took the arsenic in order to keep strong, though he had never suffered from ill health. He said he had never experienced any bad effects even when he first began using it, that he had at first taken rather less than a grain every fortnight, that he now took it twice a week, and that on omitting to take it for any longer period, he experienced a longing for it, which was relieved by a repetition of the usual dose. His reason for taking the orpiment instead of the white arsenic was that it was more easily procured; but having professed himself quite indifferent whether it were arsenious acid or the sulphuret, Dr. Knappe produced a paper containing the former (of which I also kept a sample), and having asked him to choose out a piece such as he was in the habit of taking, it was weighed and found to be nearly five grains; we had no finer weight than one grain, but the piece of arsenic was much over four, though less than five. Dr. Knappe having carefully ground this to powder on a clean piece of paper, it was transferred to a small piece of plain white bread, about as large as a man's thumbnail, and this the doctor put into his mouth; Schober chewed it and swallowed it, and then swallowed another portion of bread the same size immediately after. This was at 9.30 A.M. He stayed with us a few minutes, but he had to return to his work, promising, however, to come back in a short while. This he did at 11.30, two hours after, and made water in my presence to the amount of what I estimated at twenty-eight ounces, into a vessel previously carefully cleaned, and the urine was put into bottles thoroughly washed by myself. Unfortunately, in the hurry of my departure, in trying to pack these bottles into my hat-box, I broke one, and thus lost part of the urine. Since my arrival in this country, I subjected the contents of the two remaining bottles to chemical analysis, adopting the distillation process of Dr. Taylor as the most convenient way of separating

arsenic from the organic matters of the urine. For this purpose the urine was carefully evaporated to dryness in a clean retort ; the nearly dry residue was covered with strong hydrochloric acid, and distilled into a well-cooled receiver. The product, amounting to about half an ounce, was a clear, feebly pinkish fluid, thirty minims of which, when treated both by Reinsch's and Marsh's process, gave very characteristic arsenical deposits.

" Schober also came the following day to see me, having taken no more arsenic since the dose which he had swallowed before me twenty-six hours previously. I again secured some urine which he passed in my presence, and this, when chemically examined as above, also yielded arsenic freely.

" CASE 2.—Joseph Flecker, aged forty-six, a muscular, healthy-looking, clear-complexioned man, a tailor by occupation, told us that he had taken hütterreich, generally the orpiment, for a period of fifteen years. He first began to do so on the occasion of the inhabitants of a house, in the neighbourhood where he lived, being attacked with fever ; and when fourteen people had died in it, and no one would enter the premises, he determined to do so, and took, as a prophylactic, about one grain of arsenic daily for three successive days, while going to the infected house, and though he said he had not felt quite well at the time, he was unable now to describe specifically what had ailed him ; but on being asked if he had ever suffered from vomiting or irritation in the stomach, he said he never had.

" The day before my interview with him, he twice, viz., at 10.30 and 3 o'clock, had, in the presence of several of the villagers of Liegist, and on one of those occasions in presence of the burgomeister, who informed me that he had seen him do it, taken a piece of the sulphuret of arsenic from his pocket, and scraped off a certain quantity of it on a piece of bread and eaten it. He brought with him a small bottle of his urine, which he stated to have been passed eighteen hours after the last of the two doses, and in which I have since found a considerable quantity of arsenic. The reason which he assigned for this public exhibition of his arsenic-eating capacities was, that it had become the subject of conversation in the village, that two strangers had come a very considerable distance to witness an example of arsenic-eating, and inquire into the practice, and that he wished to make open demonstration of his assertion that he was capable of tolerating a considerable dose of arsenic. When he first came to me, he seemed somewhat unwilling to take a dose that day, owing to his previous performance, and seemed to fancy it possible that he might have some slight irritation of the stomach, such as a feeling of warmth accompanied by thirst. He did not appear to be able to give any reason for anticipating this result ; perhaps he intended it as a gentle hint that the thirst might require assuaging ; at all events, having been informed that he should not want the wherewithal to quench it—(he confessed to being by no means abstemious in the matter of alcoholic potations), he, to satisfy our curiosity, picked out a piece of arsenious acid, from the same parcel that had been shown to Schober, and which, on being weighed, was found to be as nearly as possible six grains. This he placed entire on a small piece of bread, and taking it into his mouth, crunched it up audibly, and in about two minutes after swallowed six or seven ounces of cold water, stating that he liked to drink immediately after swallowing a dose, and on such occasions preferred water. I then made him open his mouth and inspected it narrowly, but found it quite clear of bread-crumbs or anything else, thus assuring myself that no jugglery could have been practised. After having swallowed the arsenic four minutes, he eructated slightly, but till he left us, a quarter of an hour after, he had no symptoms of any bad effect. The six grains were taken at 11.30, and at 12.15 he returned, and passed a small quantity of light-coloured urine. Nearly the whole of this

was bottled for exportation, and the twelve ounces thus secured were treated by the process of distillation above described, and also yielded a characteristic deposit of arsenic.

"Flecker gave me the following account of his use of arsenic. He stated that he generally takes about the quantity we saw him swallow, once a-week, but with variations in the intervals, there being sometimes four days only, sometimes eight days between the doses. That when he has a distance to walk to his work, he takes a larger dose, and is then in good spirits for about eight days. That if he, however, intermits it for fourteen days, he feels stiff in the feet, with general lassitude and a craving for another dose. If his victuals are hard of digestion, he takes a dose to assist the stomach, and if he takes a rather full dose, he brings a good deal of wind off his stomach, but never vomits. He stated that his father had taken arsenic before him, and in considerable quantity, and that in the immediate neighbourhood of Liegist numbers use it, several taking it daily, and many in larger doses than he. He said that all who take it are healthy,—that he never knew of any one vomiting from its use, and he believed that, like the use of tobacco, if the dose is very gradually diminished, an arsenic-eater can break himself of the habit.

"One of the objections which has been made to the acknowledgement of the reality of arsenic-eating is, that the substance swallowed has not been ascertained by chemical examination really to be arsenic. This link in the chain of evidence I am able to supply. The white substance which I saw Schober and Flecker swallow, part of which I have now in my possession, is pure arsenious acid. It sublimes into octohedral crystals, and leaves no appreciable residue. The yellow substance which Schober used is a fair sample of the orpiment of commerce, and contains, as that substance usually does, a considerable proportion of free arsenious acid."

ART. 10.—*On the Relative Mortality of London and Paris.*

By Mr. TITE.

(*Lancet*, October 15, 1864.)

At the recent meeting of the British Association for the Advancement of Science, a paper was read by Mr. Tite upon a subject of much interest to the medical profession of the two more important cities of the world. The subject in question was the relative mortality of London and Paris. It was shown that researches into the rates of mortality in the latter city are surrounded with doubts and difficulties, and that its official statistics cannot be compared with those of London further back than the year 1850. If the two cities be compared as regards their geologic, political, and social conditions, considerable contrasts will be seen. Some results are in favour of one metropolis, some in that of the other. All must influence the death-rate, and the final result is, that more people die in Paris than in London proportionally to the population. If the geologic position of the French capital be regarded, it must be confessed that the nature of the soil, the character of the materials used in house-building, and all that tends to render out-door life agreeable, are

much superior to what they are in the capital of England. Paris is situated upon tertiary limestone, London on the impervious clays of the same formation. The former is, consequently, dry; the latter damp through a considerable portion of the year. Whilst the surface-waters of the one are enabled to flow off from the ground, or are quickly and well absorbed by it, those of the other remain upon its superficies. The situation, too, of Paris is higher above the level of the sea than that of London, and the strata that occur between the levels of the former city are all of them highly permeable, being mostly calcareous, excepting where they are covered with the marls and clays of the gypseous formations, or occasionally capped with the clays and gravels of the drift period. The conditions of climate are favourable to the production of animal food and the growth of cereal crops, and on the whole it is probable that the character and state of the food consumed in Paris are, in some essential respects, better than those which are to be generally met with in London. Yet notwithstanding these advantages, the mortality of Paris has been greater than that of our own metropolis for the ten years from 1853 to 1862 inclusive. In the former it has been 2·78, and in the latter, 2·39 per cent. In so far, then, as the hygienic conditions of the two capitals are concerned, London must enjoy certain great advantages over its apparently more favoured neighbour, and which may be expressed by the fact of the death-rate being about $1\frac{1}{2}$ in 1000 higher in Paris than in London. That this difference in the rate of mortality must have been greater formerly than it is now there can be little doubt, for a marked decrease in the mortality of the former city has certainly taken place since the year 1853, or shortly after the Emperor began to urge on municipal authorities the necessity of making better provision for the sewerage, water-supply, street-ventilation, and domiciliary reforms. But why, it may be asked, does there still continue an excess of mortality in Paris over London, notwithstanding the advantageous conditions we have alluded to, and the great improvements which have been made? Because, says Mr. Tite, of the dense crowding that takes place in the houses which the population of the capital of France occupy. Because of the bad hygienic conditions of these houses, so far as ventilation, the removal of refuse, and the supply of water are concerned. Because of the bad laying-out of the town originally. We are told that in Paris there were as many as 35·17 persons per house in 1856, whilst in London, 7·72 is the utmost that exists. Further: "Ventilation in Paris is very bad; in almost all the court-yards in Paris, or elsewhere in French cities, there is a 'villanous smell' that must interfere with the healthy exercise of some of the most important functions of the body." As to the drainage and water-supply of the French capital, they are yet but in a rudimentary state. The sewers act simply to relieve the streets from the rain-water, and the supply of water is so scanty that in no case is it possible to command anything like what we call a "high service." There is no water to the private drains, and great care is taken to exclude from the sewers all that we take so much trouble to throw in, and which they retain

to decompose under the houses in cesspools, these being emptied from time to time.

"There is a singular subject connected with the rate of mortality in Paris that merits careful analysis of the returns. It is, that the population that would seem to be benefited by the changes that are being made in that city are apparently subjected to new forms of disease, that go far to compensate for the mortality arising from the old forms they were exposed to, and which are now disappearing. Thus there have been observed in Paris of late to have occurred many more deaths from intermittent fever than were wont to take place in that city, and the number of fatal cases of pulmonary complaints has been considerably increased; the former are attributed to the exhalations that arise from the ground that is disturbed in erecting the new streets; the latter to the increased draught, and to the workmen breathing more dust than they were accustomed to."

The experience of our best hygeists, both of France and England, leads them daily more definitely to the conclusion, that the crowding of human beings within narrow limits, accompanied by defective ventilation, is certain sooner or later to result in devastating disease, no matter how well fed and cared for in other respects such persons may be. No doubt "over-crowding" in Paris exists to an extreme degree, but we ourselves are bad enough. Our sanitary reformers are fully alive to this, if the people themselves are not.

AET. 11.—*On the Deleterious Effects of Aniline on the Workers who prepare it.*

By Dr. KREUSER, of Stuttgart.

(*Revue de Thérap. Médico-Chirurg. ; and Edin. Med. Journ., Aug. 1864.*)

The author observed in the Catherine Hospital of Stuttgart several cases of intense bronchitis, characterized by a violent dry spasmodic cough, accompanied by ulcerations, situated especially on the scrotum and lower extremities, of a rounded form, with borders sharply cut, often callous, covered with thick black crusts, under which was a base of a dirty grey colour, the surrounding parts being swollen and painful. All this yielded readily to a simple treatment so soon as the workmen were removed from the manufactory where the colour was prepared. If care was taken to protect the skin by suitable clothing, by making the workmen wash frequently, and by improving the ventilation, these bad effects might be avoided. The existence of this peculiar affection has been also observed by M. Stadler at Marbourg, and M. Stoerig at Wildungen.

(B) CONCERNING ACUTE DISEASES.

ART. 12.—*On Miasmatic Typhoid Fever.*

By Dr. J. J. LEVICK, Physician to the Pennsylvanian Hospital.

(*American Quarterly Journal of Medical Science*, April, 1864.)

After some introductory remarks, Dr. Levick says:—

"After having been accustomed for many years to treat the ordinary form of typhoid fever, my attention was arrested in the early part of last year by the occurrence of a series of cases which, while presenting the ordinary phenomena of typhoid fever, had superadded to these many of the symptoms of well-marked miasmatic remittent fever. These cases were characterized by the suddenness of the attack, by the intense pain in the head and back of the neck, by the regularly periodical remission and exacerbation of the fever, and by its amenability to quinia. I have ventured to call such cases *Miasmatic-typhoid fever*, preferring this term to that of *malaria*, for although etymologically the distinction will not hold, yet by common consent in this country the term miasmatic is accepted as belonging to that form of poison which is believed to be due to paludal exhalations; while the term *malaria* (*bad air*) applies with especial propriety to what is now regarded by some English authorities as the specific cause of typhoid, or, as they call it, pythogenic fever.

"In some remarks made at the Pennsylvania Hospital, June 7th, 1862, which were published at that time, I gave a detailed clinical account of one group of these cases. I shall therefore content myself here by giving, in a condensed form, a summary of the observations then made. That these were cases of typhoid fever was shown by the fact that in every instance, even in the mildest, was found the characteristic eruption. That there was disease of Peyer's glands was inferable from the fact that diarrhoea was present in every case, or was readily induced by small doses of cathartic medicine, a teaspoonful of castor-oil producing five alvine passages.

"In some of these cases the attack was gradual, a sort of imperfectly developed stage existing for a week or longer; the patient able to move about, but with a little diarrhoea; pretty well in the morning, but with severe headache and some fever every evening, with restless, uncomfortable nights. In other instances the attack began suddenly, with a chilliness and intense 'splitting' pain in the head, unlike the headache of a typhoid fever, which is dull and heavy. A slight cough, with the usual bronchitic râles, was noticed in nearly all the cases referred to. The remittent character of some of these cases in their forming stages has been already alluded to; in all of them the paroxysmal character was strongly developed, a marked exacerbation occurring in the latter part of the day or evening, the fever passing off towards morning, and the apyrexia ushered in with as profuse sweating as is witnessed in the most decided cases of miasmatic fever. I am well aware that a partial

subsidence occurs in ordinary typhoid fever, but the exacerbation and remission of the miasmatic typhoid fever bear no resemblance to this temporary subsidence in 'continued fever.' The severe headache, which has already been noticed, was one of the most constant symptoms—described by the patient as 'splitting,' 'battering,' and the like; it appears to have been one of the chief sources of his discomfort, though generally associated with it were intense pains in the back of the neck and the muscles between the shoulders, and in some instances universal achings. The tongue was generally covered with a loose yellowish-white fur, and in all the cases to which I now refer did not present the dry, brown condition which is so often seen in the advanced stages of typhoid fever. The pulse, though in two instances reaching 120, was rarely above 90, and in all was soft, and in none alarmingly feeble. There was in these cases no great tympanitis. The mental condition of these patients was unlike that of ordinary typhoid fever, in being free from the dulness or hebetude which usually attends this disease. Though indisposed to any active mental exertion, all these patients retained their interest in the events transpiring around them, and gave intelligent responses to questions put to them. In none of them was there well-marked delirium. These were the phenomena which presented themselves in all the cases of miasmatic typhoid fever which were unassociated with the graver complications hereafter to be alluded to. Merely considering the facts which have been mentioned, we might readily infer that this exposure to the double poison of miasmatic and typhoid fevers had developed a new disease, a *tertium quid*, as it were, in which the elementary constituents were so intimately united as to have developed a new and distinct compound body, analogous to what occurs in chemical combinations. But it is an interesting fact in this connexion, that the administration of the usual remedy in remittent fever, sulphate of quinia, while it always promptly relieved and arrested the miasmatic fever element of the disease, did not promptly cure the patient; it mitigated the symptoms, but convalescence did not begin for a week or more; the enteric fever element continued, and this disease ran its course. But what was also highly interesting was the fact that the addition of the miasmatic fever element did not render the disease more serious than typhoid fever in its ordinary form. On the contrary, either this complication, or the means taken to combat it, seemed to render the disorder more tractable and less fatal. It should before this have been mentioned that all these illustrations of miasmatic typhoid fever occurred among soldiers or sailors engaged in our national service—sailors who had been attached to vessels carrying troops to Fortress Monroe and the coast of North Carolina; soldiers in the army of the Potomac, before and at the beginning of the Peninsular campaign.*

* "More recently my attention has been attracted to another interesting fact in connexion with this form of disease, which may be here briefly alluded to. During the late invasion of our State, it is well known that large numbers of patriotic young men entered the ranks of the State militia, and

" Such was the character of the cases of miasmatic typhoid fever coming under notice in the spring and early summer months of last year. On the 7th of July, 1862, there arrived at the Pennsylvania Hospital, direct from the army of the Potomac, twenty-seven cases of fever presenting very different phenomena. These were men who had accompanied the army in its march along the Peninsula; had lain before Yorktown, had toiled in the swamps of the Chickahominy, and either had been active participants in the seven days' fight, or had been in the hospitals at the White House and elsewhere in this vicinity. They represented almost all forms of typhoid fever. Some were cases well described in my previous remarks, though of a more severe type. Others were instances in which, with the ordinary phenomena of typhoid fever, the tache, diarrhoea, &c., were associated those of pernicious congestive remittent fever, as evinced by the shrivelled washerwoman's fingers, copious serous discharges, not unlike those of Asiatic cholera, the colliquative sweats, and other symptoms of extreme exhaustion. Still another class were individuals in whom, to the ordinary typhoid fever symptoms, were added those of great blood-poisoning, cases presenting the true typhoid, *i. e.*, typhus-like symptoms, as shown by extreme emaciation, excessive nervous tremors, delirium, sordes, excessive irritability of stomach and vomiting of a dark greenish fluid, diarrhea, involuntary discharges from the bowels, petechiae, vibices, sloughing bed-sores, haemorrhage from the bowels, and in a large number of cases ending in death. The contrast between the mortality of ordinary typhoid fever, and as thus modified, was strikingly impressed on the writer's mind by the fact that of twenty-nine cases of typhoid fever treated by him during the

endured the hardships of a brief but victorious campaign. The health of these soldiers during their active service, for the most part, was excellent; but a week or more after their return very many of them were seized with this fever, and in some I am told it proved fatal. A more remarkable illustration of the same occurred to the members of Hasting's Battery, of this city. These young men offered their services to the President in the summer of 1862 for one year, which were accepted. They were attached to the army of the Potomac, and shared its varied fortunes, hardships, and exposures. At the expiration of the twelve months they were mustered out of service. Up to this time the health of the regiment had been excellent. But on their return to their homes in Philadelphia, numbers of them were seized with a modified form of typhoid fever, which, though grave, has I believe in no instance proved fatal. Andral, in his earliest account of typhoid fever, makes mention of the fact that medical students coming to Paris for the first time were very liable to be attacked within a few weeks of their arrival with typhoid fever. The same has been noticed in London. Every one who has had much to do with students of medicine coming to our city from North Carolina and other miasmatic regions, well knows that nothing was more common than for them to be attacked a fortnight or sooner after arrival here with a severe form of bilious remittent fever, and this too after having lived safely all their lives long in the midst of the miasmatic poison. It is at least an interesting coincidence that the same fact obtains in the form of fever which we are now considering."

months of April, May, and June, uncomplicated, except by those lesions which usually occur, all recovered; while of twenty-seven cases of typhoid fever brought under his care from the army, in which the complication existed of miasmatic poison, the scorbutic diathesis, the typhous condition, nine, i. e., one-third, died—four of the number not living long enough to be put on a regular treatment.

"Indeed it is difficult to paint a more melancholy picture than that which these poor creatures exhibited. Brought in crowded transports, they arrived in our city during the intense heat of July, and thence over the rough stones were conveyed to our hospital. With constitutions broken down by the life they had recently led, necessarily deprived of the kind of food to which their previous lives had accustomed them, drinking the water of the James, or the viler water of the Chickahominy, wearing both by day and night the same apparel; ill in crowded hospitals, to be transferred to more crowded transport-ships—nothing was wanting to fill up the catalogue of causes which result in the development of genuine typhus fever. And yet I cannot recall one case in which the true typhus fever rash exhibited itself, nor was there, so far as I could learn, a single instance in which the fever was communicated by contagion to any one in the hospital. Petechiae, passive haemorrhages, there were in melancholy abundance, but not the true rubeoloid rash. And yet, that the condition of the blood was very much that of true typhus, there can be no doubt; the injected eye, the acute delirium, or the profound coma, all corresponded to this, as well as did the other phenomena before described. There was, indeed, in these cases a third noxious agent—a scorbutic or typhous condition of the blood associated with the true typhoid and miasmatic poisons.

"Those who are familiar with the celebrated Walcheren fever, cannot fail to be struck with the remarkable correspondence, I had almost said identity, existing between this and the fever of the Chickahominy."

ART. 13.—*On the Latent Period of Scarlet Fever.*

By Dr. MURCHISON, Physician to the London
Fever Hospital, &c.

(*Lancet*, August 13, 1864.)

In an excellent paper on the etiology, pathology, and treatment of scarlet fever, Dr. Murchison, writing upon the debated subject of the duration of the latent period of the disorder, says:—

"It is a point of no small importance to fix with accuracy the latent period of the several infectious diseases. That of small-pox has been most studied, and is found to be subject to but little variation. Advantage is taken of this knowledge in the practice of vaccination. The development of the areola which is necessary for the protective effect of vaccination occupies nine days; but as the latent period of small-pox is twelve or fourteen days, it follows that, if a person exposed to the poison of small-pox be successfully vaccinated *at once*, he has time to escape.

"The latent period of scarlet fever appears to be shorter than that of the other infectious diseases. Most observers have agreed in fixing it at between two and seven days, as follows: Dr. Binns, two days; Withering, three or four days; Bateman, three to five days; Heberden, Gendrin, and Frank, four days; Cazenave and Schedel, three to six days; Dr. Watson, four to six days; Willan, never more than six days; Guersant and Blache, two to seven days; Dr. Copland, two to eight days. The writers now cited, however, do not state the facts on which their opinion is founded. In the cases alluded to by Rostan, where the poison was introduced by inoculation, seven days are said to have elapsed before the appearance of the eruption. It must be remembered, however, that when small-pox is inoculated, the latent period (seven days) is not the same as under ordinary circumstances (twelve days). A few cases have been recorded in which the latent period has been said to extend to three or four weeks, but it may be doubted if some of the cases referred to were really examples of scarlet fever, and in others, if the poison had not been lurking in the clothes rather than in the bodies of the individuals who were subsequently attacked.

"In some of the cases where I have succeeded in ascertaining the latent period of scarlet fever, its duration has been shorter than might be inferred from any of the authors cited above. Considering the difficulty of collecting information on the subject, and the fact that cases suitable for the investigation rarely present themselves, the details of my observations appear to me worthy of being recorded."

CASE 1.—A female aged twenty-two was admitted into the London Fever Hospital on April 22nd, 1858, on the third day of an attack of scarlet fever, which proved fatal on the eighth day. On the 19th of April she had come from a part of Oxfordshire where scarlet fever was not prevalent, to see her sister in London, who was dangerously ill with the disease. On the following morning she was seized with rigors, headache, and sore-throat, and on the 21st a scarlet rash was observed on the skin. In this case the latent period was evidently less than one day.

CASE 2.—Ann W—, aged twenty, a servant, was admitted into the London Fever Hospital, suffering from scarlet fever, on Oct. 5th, 1863. On Sept. 28th, a young lady had come from school to the house where Ann W— lived, ill with scarlet fever, and was attended on by her the same day. The next day Ann W— was seized with sore-throat, followed by the usual symptoms of scarlet fever. Here also the latent period did not exceed one day.

CASE 3.—In this case, for the details of which I am indebted to Mr. Marson, of the Small-pox Hospital, the latent period was about thirty hours. Some years ago, a family in Gray's-inn-lane gave a children's party. Before the day one of their own children took scarlet fever. Not wishing to put off the party, the sick child was removed to a garret. The party took place on a Monday, the children being at the house from about four to nine P.M. On the Tuesday between eleven and twelve P.M., a child from a distant part of London, who had attended the party, was seized with the usual symptoms of scarlet fever, and on the Friday morning she was dead.

CASE 4.—Mary B—, aged twenty-one, was admitted into the London Fever Hospital on Sept. 14th, 1863—on the fourth day of an attack of

scarlet fever. She was in service : and on the evening of Sept. 9th she had gone to "a new place," at a house in which certain of the inmates were suffering from scarlet fever. On the morning of the 11th she was seized with vomiting and sore-throat, followed by all the ordinary symptoms of scarlet fever.

CASE 5.—On March 4th, 1863, about four P.M., W. F—, aged eleven, a schoolboy, went home with some of his schoolfellows to a house in which there were cases of scarlet fever, and remained there about two hours. On the morning of March 6th he was seized with scarlet fever, which was followed by gastro-enteritis, terminating fatally on March 23rd.

CASE 6.—Miss B—, aged twelve, returned to school at Kensington on Oct. 5th, 1858. A girl at the school was ill at the time with scarlet fever. On Oct. 8th, Miss B— felt feverish and had a sore-throat ; and on Oct. 9th the rash of scarlet fever made its appearance. The disease ran its usual course, and terminated favourably.

CASE 7.—T. M—, an officer in the Limerick Artillery, aged twenty-two, arrived in London on Sept. 2nd, 1858. On Sept. 5th, in the afternoon, he visited a friend whose little girl had scarlet fever, but so slightly that she was not confined to bed. He took the girl on his knee, and kissed her. On the morning of Sept. 8th he was quite well ; but towards evening he was attacked with headache, heaviness, and sore-throat, followed by a dusky scarlet rash, ulcers on the tonsils, constant delirium, sleeplessness, and great prostration. He died on Sept. 14th, at eleven A.M.

CASE 8.—Susan W—, aged sixteen, was admitted into the London Fever Hospital on Jan. 13th, 1863, along with her sister, who was suffering from typhus. Susan W—'s febrile attack terminated on the seventh day with an eruption of herpes on the lips. She never had any typhus eruption ; and on Jan. 16th her pulse was 60, and her appetite good. In the night of Jan. 16th she began to complain of pains in the limbs and sore-throat ; and next day the rash of scarlet fever had appeared, and the pulse had risen to 108. Although this patient was admitted into the typhus ward, she was attended on by a nurse from the ward devoted to scarlet fever.

CASE 9.—Ellen N—, aged four, was admitted into the London Fever Hospital on May 2nd, 1863—on the fourth day of an attack of scarlet fever, which proved fatal from pneumonia on the eleventh day. This child had been living in a street where scarlet fever was not prevalent ; but on April 25th she was taken to the workhouse of St. George's-in-the-East, and on the same day she played with a child in the workhouse, who was sickening with scarlet fever, and who was admitted into the London Fever Hospital with the disease in a malignant form on April 27th. Ellen N— was taken ill with headache and sore-throat on April 29th, and the scarlet rash appeared on April 30th.

CASE 10.—Robert W—, aged forty-four, was admitted into the London Fever Hospital on Sept. 17th, 1858, on the fifth day of scarlet fever. On Sept. 8th he went to act for a coachman who, with his children, was laid up with the disease. He did not sleep in the house, and never entered the sick room ; but on Sept. 8th the coachman, though ill, came out to see him and gave him a cup of tea out of his hand. On the morning of the 13th he fell ill with scarlet fever, the rash appearing on the 14th.

CASE 11.—Thomas B—, aged sixteen, was admitted into the London Fever Hospital on Oct. 15th, 1858, on the fifth day of scarlet fever, and on the eleventh day he died. Four and a half days before his seizure he arrived from sea and went to his mother's house, where scarlet fever was. Three of the family had died of it, and a fourth was not expected to live.

CASES 12, 13.—On Jan. 10th, 1864, a girl returned to the Sailors' Home at Hampstead from visiting a house where scarlet fever was. She did not

take the fever herself, but on the 15th another girl was attacked, and on the 16th a second case occurred. The only discoverable source of the poison is that here stated.

"These are all cases in which I have succeeded in tracing the latent period of scarlet fever. It will be seen that in some this period only extended over a few hours, and that in none did it exceed six days. In all of them we are able to fix the limit of longest duration of the latent period : thus—

"In Case 1 it was less than 24 hours

"	2	"	24	"
"	3	"	31½	"
"	4	"	36	"
"	5	"	40	"
"	6	"	3	days
"	7	"	3	"
"	8	"	3½	"
"	9	"	4	"
"	10	"	5	"
"	11	"	5	"
"	12	"	5	"
"	13	"	6	"

But in a few only of the cases is it possible to determine the shortest limit, as the patients remained within the sphere of the poison from the moment of their first exposure up to the date of their illness. This is a point often lost sight of in investigations of this sort. In only four of the cases are we able to fix the actual moment of infection with anything approaching to precision.

In Case 3 it was between 26½ and 31½ hours

"	5	"	38	"	40	"
"	7	it was	"	3	days	
"	10	"	"	4½	and 5	days.

"The above cases lend no support to the opinion expressed by some writers, to the effect that the latent period is longer in adults than in children.

"In the only case where Professor Troussseau has been able to trace the period of incubation of scarlet fever, it did not exceed twenty-four hours. The case is remarkable. An English gentleman with his daughter was returning from Pau to London, and was joined at Paris by another daughter who came direct from London. Scarlet fever was prevalent in London, but there was not a case of it at Pau. The second daughter was seized with scarlet fever in crossing the Channel, and joined her relatives in Paris seven or eight hours later. She occupied the same room in the hotel as her sister, who was also attacked within twenty-four hours. Dr. Richardson relates that once when he was attacked, the effect of the poison was instantaneous. He had applied his ear to the chest of a patient suffering from scarlet fever, and was conscious of a peculiar odour emitted by the patient. He immediately felt nauseated and chilly, and from that moment he was able to date the commencement of his illness.

"It follows from the evidence now adduced, that the latent period

of scarlet fever varies from a few minutes to five days, and that it rarely, if ever, exceeds six days."

ART. 14.—*On the Condition of the Stomach and Intestines in Scarlatina.*

By Dr. SAMUEL FENWICK.

(*Proceedings of the Medico-Chirurgical Society, June 28, 1864.*)

The object of this paper is to prove the following propositions:—

1st. That the mucous membrane of the œsophagus, stomach, and intestines is inflamed in scarlatina.

2nd. That desquamation of the epithelium of these parts takes place.

3rd. That notwithstanding the anatomical changes in the mucous membrane of the stomach, the formation of pepsine is not prevented.

4th. That the condition of the skin is similar to the condition of the mucous membrane in scarlatina.

In support of the first proposition, the microscopic examinations of the mucous membranes of the œsophagus, stomach, and intestines are detailed in ten cases of death from scarlatina during the first week of illness, and in six cases who died in the second and third week of the fever. The first effects of the scarlatina poison upon the mucous membrane of the stomach are shown to be the congestion of the bloodvessels and the stripping the epithelium from the tubes and the surface of the organ, and also the softening of the tissues. The tubes are greatly distended by granular and fatty matters, or by small cells intermixed with granules, and in some cases they are lined by a newly-formed membrane. Sometimes no normal cells can be distinguished; in other cases they are present, but are scattered irregularly. After the second or third week the tubes are found less distended than at an earlier period, and whilst their closed ends are still loaded with granular matters, which greatly obscure the gastric cells. These become more evident towards the surface of the mucous membrane. The cells at this period are sometimes very large, sometimes loaded with fat or coated with granules, and seem to have but little adhesion to their basement membrane, as they readily separate from the tubes, but adhere closely to each other. The effects of the inflammation upon the intestines seem, in slighter cases, to consist in the effusion of granular and fatty matters into the mucous membrane; but in more severe cases the tubes of Lieberkühn are obstructed by epithelial cells, whilst extravasations of blood take place in the villi, and these, with the rest of the mucous membrane, are loaded with small cells and granules. In one case the mucous membrane was entirely stripped of villi, excepting a few fragments which still remained, and the enlarged and prominent openings of the follicles of Lieberkühn gave its surface the appearance of a sieve. In some instances in which the pancreas has been examined, evidences of disease presented themselves.

The second proposition is stated to be more difficult of proof, inasmuch as vomiting usually occurs only in the first stage, and the author had no opportunity of examining the vomited matters at this period of the disease. In one case, in which vomiting took place in the third week, fibrinous casts of the stomach-tubes were discovered, and inflammation of the mucous membrane was proved to have existed by post-mortem examination. The chief reason upon which the opinion that desquamation of the epithelium occurs was founded, was from the microscopic examination of the contents of the stomachs of those who had died of this disease. The contents in recent cases consisted of pieces of fine membrane, of cells, and of granules and shreds of membrane. The membranes were of the shape and size of the tubes of the stomach, and were covered with granules and fat. The cells varied from $\frac{1}{200}$ to $\frac{1}{200}$ of an inch, and were usually fringed with fine pieces of membrane. In cases of longer duration the membranes were covered with cells, and were also of the size and shape of the stomach-tubes. In order to ascertain if these appearances were trustworthy as evidences of inflammation, the contents of the stomachs of forty-five subjects were examined at the Middlesex Hospital, the condition of the mucous membrane being at the same time noted. In only one were there any fibrinous casts, and it was in a case of acute gastritis. In eighteen there were only separate cells, chiefly of the columnar form, and in none of these was there any inflammatory action. In eight cases casts of the upper parts of the tubes were plentiful, composed only of healthy conical cells, and in all the mucous membrane was in a natural condition. In eighteen there were either plugs formed of cells and granules from the secreting parts of the tubes, or the casts of conical cells were overlaid with granular matters, and in all of these the stomach was more or less inflamed. Two cases of gastritis, unconnected with scarlatina, were also quoted as examples of the forms in which casts of the stomach-tubes appeared in vomited matters during life, and the author stated he had detected casts of the stomach-tubes in matters vomited by persons affected with gastritis connected with diseased kidneys, with inflammatory dyspepsia, and other forms of inflammation of the gastric mucous membrane. It was urged that if casts of the gastric tubes can be discovered during life in cases of gastritis, and if in scarlatina this condition exists, and casts have been found in the stomach after death, there is every probability that desquamation of the epithelium takes place in this organ, as it does in the skin and the kidneys.

In support of the third proposition, the results of the following experiments are given in three cases of scarlatina:—Ten grains of hard boiled white of egg were digested at a temperature of 90° for twelve hours in an infusion of the mucous membrane, to which three per cent. of hydrochloric acid had been previously added. The average loss of albumen was three grains and two-thirds. Similar experiments performed with the stomachs of eleven males who died of various diseases at the same hospital gave an average loss of four grains; so that there had been scarcely any diminution of pepsine produced by the fever. As a contrast to this were the results of

similar experiments upon four cases who died of typhus fever. In two of these the albumen had gained three grains of weight by imbibition, and was not at all softened ; whilst in the other two it was softened, and one had lost only half a grain, the other one grain and a half in weight. But as the activity of the digestion must depend not only upon the relative amount of pepsine, but also upon the bulk of the mucous membrane, this was also attempted to be estimated. The average weight of the mucous membrane of the stomachs of ten males dying of various diseases at the Middlesex Hospital was eighteen drachms, the weight of two recent cases of scarlatina was eighteen and sixteen drachms (the latter being in a boy), whilst it only amounted to fifteen drachms in one who died in the third week of illness. In four cases of typhoid fever the average weight of the mucous membrane only reached eleven drachms.

Under the fourth proposition it is stated that the skin had only been examined microscopically in three cases. In the first, in which the patient died after a few days' illness, the only morbid appearance in the cutis was an occasional minute extravasation of blood in the neighbourhood of the sudoriferous ducts. The rete mucosum was greatly thickened, and numerous round cells with large nuclei were everywhere visible, intermixed with the natural cells. The basement membranes of the sweat-glands were thickened, and the epithelium lining them was so much increased that in most cases it obstructed their channels. In some of the sweat-glands the coils of which they were composed were loaded with coagulated blood, and were greatly and irregularly distended. In the other recent case the appearances were similar, excepting that the external layers of the cuticle were stained with blood in minute patches, and the sweat-ducts were also reddened ; but there were no extravasations of blood either in the glands or cutis. In some of the ducts the epithelium was detached from the basement membranes. In the case of a man who died during the third week the sudoriferous pores were still choked up, but in the glands the epithelium seemed in many places to be torn away, leaving the basement membranes bare, or only covered by ragged particles. The cutis was in a natural condition.

The author states that although he has described the appearances of the skin and mucous membranes as the results of inflammation, yet that certain considerations suggest the idea that the term when so used is perhaps misapplied. In scarlatina, we find that in each part the morbid condition is mostly confined, in the first instance, to the basement membranes, and consists in the formation of layers of new cells, which, in the skin, are transformed into cuticle of natural appearance, and in the stomach contain pepsine. If future researches should prove that a similar condition occurs in the kidneys and other parts, it will be necessary to look upon the structural changes produced as resulting from increased physiological, rather than from pathological action ; and that the primary effect of the scarlatina poison is suddenly and violently to stimulate the natural cell-growth of the various secreting organs.

ART. 15.—*On Rubeola Notta.*

By Dr. BABINGTON.

(Lancet, May 7, 1864.)

Dr. Babington brought under the notice of the Epidemiological Society not long ago an eruptive disorder, or exanthem, of an anomalous character, which has within the last few weeks been so prevalent, at least in London, as to entitle it to the designation of an epidemic. He has himself seen several cases, and heard of many more. It is a papular eruption, in many respects resembling rubeola, but distinguished from it in that the papulae are less distinct, not arranged in crescentic clusters, and do not appear on the limbs, or, at least, but very rarely and slightly. There is also more constitutional disturbance in proportion to the extent of the eruption, and the convalescence is more speedy and complete. It differs from roseola by not being symptomatic of any other disorder, and by being papular in character, the patches, too, being more circumscribed and duskier in hue. There is also more prominent congestion of the head, and the disease runs a more definite course. From scarlatina the eruption differs in being essentially papular, and in being unaccompanied by any prominent soreness or ulceration of the throat. The prognosis of the affection is favourable, and so far as it is yet known it is rarely or never fatal. From the general resemblance of this eruptive epidemic to measles, Dr. Babington proposes to designate it *Rubeola notha* (illegitimate or bastard measles). In respect to treatment, Dr. Babington stated that the feverish symptoms accompanying the malady are best met by cooling salines, diluent beverages, and a light diet. Gentle aperients are occasionally required, but during the eruption spontaneous diarrhoea sometimes occurs. The debility during convalescence, which is considerable, indicates tonics and stimulants. Dr. Babington recommended the Society to call for further information on the subject of the epidemic, remarking "that the disorder, though comparatively unimportant in itself, deserves to be observed accurately; and that it is only through the independent efforts of many that a comprehensive knowledge of any disease can be obtained." He added, however, that it is quite possible that this seemingly trivial disorder may be the precursor of some other of graver import.

The designation proposed by Dr. Babington will meet a difficulty which has been felt by practitioners who have had opportunities of observing this epidemic, and who have been at a loss how to name it. The term very happily characterizes the affection, and will doubtless meet with general acceptance.

ART. 16.—*On Diphtheria.*

By Dr. HILLIER, Assistant-Physician to the Hospital
for Sick Children, &c.

(*Medical Times and Gazette*, August 20, 1864.)

This paper was read at the recent annual meeting of the British Medical Association. Its object is to set forth the history of diphtheria as obtained at the Hospital for Sick Children in London, during the past seven years.

The author states that, although not commonly recognised in England, it is the opinion of many eminent physicians that cases of this disease have at intervals come under their notice ever since they had been in practice, but had not been understood. He gives the statistics of diphtheria as it has been met with in the hospital. He mentions that it has occurred as a primary disease, or as secondary to measles, scarlatina, Bright's disease, or surgical operations. The secondary cases were more fatal than the primary.

The larynx was involved in twenty-four of thirty-four cases; the symptoms of laryngeal complication made their appearance in eleven cases on the first day, and in all except two within the first week from the appearance of other symptoms. In one case the larynx was first involved on the nineteenth day. Albuminuria was observed in nearly all the severe cases; of twenty-four cases in which the urine was frequently examined, it was present in nineteen and absent in five. It made its appearance never earlier than the fourth day; once on that day, twice on the fifth day, once on the sixth day, once on the seventh day, five times on the ninth day, once on the thirteenth day, and once as late as the nineteenth day. At the same time that albumen was present in the urine the amount of that secretion became more and more scanty, until it was sometimes almost suppressed. The presence of albuminuria and scanty secretion of urine is stated to be a more common occurrence in diphtheria than in scarlatina. Dropsy was not once observed. A tendency to somnolence was frequent, convulsions rare. *Post-mortem facts:* False membranes, more or less tenacious, were either found on the fauces, tonsils, pharynx, epiglottis, larynx, trachea, or bronchi, or on all these parts at once. In seven cases ulceration of the mucous membrane was present, and in nearly all the epithelium appeared to be the seat of the exudation. The kidneys exhibited to the naked eye increased opacity of the cortex, and in some cases undue injection. Microscopically, there was distension of the tubuli with epithelial cells, which were too granular in aspect, and separated too easily; in some cases there was granular deposit between the Malpighian capsules and the glomeruli. The changes indicated incipient nephritis, very similar to what is found after scarlatina. In three cases which lingered nearly three weeks, distinct fatty degeneration of the heart was found; these cases all died of asthenia, the throats being nearly or quite well.

The author has been struck with the very small number of cases

in which paralysis or nervous symptoms have followed diphtheria in the hospital; these signs occurred in not more than three cases altogether, and these not of a severe or general kind.

He is inclined to think that these symptoms are more common amongst adults than children, and amongst those in easy circumstances than the poor. In private practice he has twice seen paraplegia, once partial amaurosis, and once paralysis of the soft palate; in three of his medical friends paralytic symptoms were observed. The cause of the paralysis was not obvious; it was not due to albuminous urine nor to obstructed respiration, but it was an evidence of the general nature of the disease. Dr. Hillier suggests its dependence on a deficient nutrition of the nerves rather than on reflex disturbance.

As to the pathological affinities of diphtheria, he is of opinion that the disease was totally different from scarlatina, but that it is impossible to distinguish it from croup as it has occurred in London during the past seven years.

Croup appears to him to be laryngeal diphtheria, and diphtheria to be epidemic croup. Most of the cases formerly described as croup would now be called diphtheria, and cases of croup are now frequently met with in which there is high fever and laryngeal obstruction from the rapid exudation of false membrane, which are called diphtheria because there is albuminous urine and convalescence, or death attended with much asthenia. Croup of twenty years ago was often accompanied with exudation on the tonsils or soft palate, and diphtheria of the present day often begins in the larynx, and the fauces were subsequently involved or entirely escaped. Albuminuria was not detected in diphtheria until lately, and since that time it has been found both in croup and diphtheria. The peculiar nervous phenomena that occasionally follow diphtheria have not been so frequent as to excite any surprise even should they as frequently have followed croup that the connexion between the primary and secondary symptoms was overlooked; or it may have been that the disease when sporadic and less virulent was not followed by these phenomena. The most powerful argument for distinction between croup and diphtheria is that diphtheria is contagious and croup has never been so regarded.

In regard to treatment, the author states that no specific has been discovered. The indications are to combat the tendencies to death. Diphtheria either kills by asphyxia from mechanical obstruction to the entry of air into the lungs, or by asthenia from the depressing influence of the general disease, or from uremia, or from cardiac failure. To counteract the first mode of death calomel in small doses frequently repeated was advocated in diphtheria as in croup, when the previous health of the patient was good and the general symptoms were not very asthenic. Chlorate of potash was also found useful. No marked benefit has been observed from the use of ice, and but little from the perchloride of iron. If calomel was not given, simple salines, with or without iodide of potassium, were recommended. A nutritious diet and wine, according to the state of the pulse, to be administered. To counteract the depressing

effect of the general disease, carbonate of ammonia, cinchona, and stimulants were indicated. A too lavish use of alcohol appeared to favour renal complication.

Tracheotomy has been resorted to in a large number of cases, and is recommended as affording a prospect of cure, and, though often failing to cure, yet being usually followed by great relief and a more easy death. In three cases at the hospital recovery followed tracheotomy, which had been adopted at a time when there was from other treatment no hope of recovery. Five other cases were reported of children in whom the operation had been successful, and of two adults. The importance of not neglecting general treatment after the operation had been performed is insisted on. Four cases are referred to in which it has been impossible to dispense with the canula for several months after the operation: in one the larynx was permanently occluded by the cicatrization of ulcerated surfaces; in another there were ulcers in the larynx; in the other two the tube has been ultimately withdrawn and the fistula had healed.

Laryngotomy is recommended in the adult, and tracheotomy in the child. It is useless to have recourse to the operation unless the constant attendance of experienced, or at any rate clever, nurses could be secured to carry out the after treatment. The tube recommended is a double one, the outer portion being bivalve and the inner having its convex aspect looking upwards.

To counteract the tendency to ischuria renalis, diluents should be freely given, but in these cases the treatment is often useless from the occurrence of obstinate vomiting, which is always regarded as a grave symptom.

ART. 17.—*On Diphtheria.*

By Mr. JOHN BRIDGER.

(*Medical Times and Gazette*, August 20, 1864.)

In a paper on this subject, read at the 22nd meeting of the British Medical Association, held at Cambridge, Mr. Bridger said:—

“ Since April, 1862, upwards of 3000 cases of diphtheria have fallen under my care. At the onset of the epidemic the disease was of a more acute character than at the present time, cases then presenting the characteristic appearances so well known to the profession, and so ably described in a lecture by Dr. W. H. Ranking, delivered at the Norwich Hospital—*vide Lancet*, January 8, 1859.

“ Latterly most of the cases have been of a milder character, very many of them relapses. The sequelæ are often very serious, such as pleurisy, pleuropneumonia, endocarditis, peritonitis, croup, paralysis, &c. &c.; in one case there was abscess of the liver, in one gangrene of the lung; some have passed membrane from the bowel, one from the bladder. Erysipelas has very frequently followed, sometimes commencing at the nostrils, and spreading over the face and head, sometimes from one or both ears, and spreading over the

head and face, in a few cases from the anus, umbilicus, penis, or vagina, and extending slowly over the whole body and lower extremities ; in the three last-mentioned points of commencement it has occurred in infants that took the disease from their mothers, and showed it either at birth or within a few days. Women who have the disease previous to or during pregnancy have their condition modified by it ; in some it produces abortion, in some premature confinement, in others haemorrhage or lingering labour ; whilst in all puerperal women, for a period of from three to six weeks after delivery, and occurring every few days, there is a discharge of shreddy membranous or glairy matter, like uncoagulated white of egg ; this has occurred in fifty-two cases, my attention being generally called to it by the patient or nurse as something unusual.

"The children born of these affected parents invariably show signs of the disease soon after birth : if you look into their throats you see the tonsils, uvula, and soft palate swollen and red, perhaps slightly oedematous, producing a slight hoarseness and cough ; this may go on for some days, or even weeks : the child, seeming neither well nor ill, all at once is taken worse, screams out with pain, and dies in a few minutes. Whenever I have been able to examine the body I find death caused by effusion into the pericardium. Altogether, out of this number of cases, there have been seventy-five deaths. Twelve were complicated with scarlet fever, nine were complicated with measles, one with confinement and scarlet fever, one with confinement alone. A primipara, a young woman, aged 21 years, gave birth to an acephalous child, and was doing well up to the twelfth day, when she complained of her throat, the tonsils, soft palate, and uvula looking red only. In about twenty-four hours she complained of a kicking sensation in the throat, screamed out, and died. No post-mortem was allowed.

"I have made twenty-four post-mortem examinations, and wish particularly to bring before the notice of the members of this Association a certain condition of the auriculo-ventricular valves of the heart seen in every case where a post-mortem was allowed, whether complicated with any other disease or not, and to the best of my belief not previously made known to the profession. It is a roughened, reddened, and thickened appearance of the valve, as if by interstitial deposit, situated midway between its insertion around the ventricular opening and the attachment of the chordæ tendineæ, one or both valves being affected more or less, according to the severity of the case and the time elapsing between the symptoms of heart affection and death. The symptoms denoting it may come on early or late, generally late, several cases having occurred where to all appearances the patient has been well, and may be diagnosed by the following symptoms :—An anxious countenance, hurried respiration, a rapid pulse (from 120 to 170), tenderness over the precordial region, scarcely any pain except on pressure. In four cases I noticed a slight murmur, systolic ; in two it subsided in a week ; one remained permanent for some months ; one case is now under treatment, and at this present time there is a distinct bruit heard at the base over the valves, and along the course of the

aorta. The duration of the urgent symptoms varies from three to seven days, the shortest being three, the longest seven days.

"I have not noted the exact number of cases in which the heart has been so affected, but can safely say it occurred in at least a hundred; in some it came on most severely, and was like a state of collapse, with cold clammy perspirations.

"In this endocardial affection I give the steel mixture with nitrate of potass every two, three, or four hours, a small dose of grey powder with compound ipecacuanha every night, and apply a sinapism twice daily until the pain and tenderness on pressure subside. In the cases with a bruit, and also accompanied by pain, gr. iiiij. of the extract of conium are given every four or six hours as long as required.

"In all cases where membrane extended down the trachea and lesser bronchi death followed rapidly. In those cases simulating spasmodic croup, with patches of membrane on the tonsile only, five have died, three recovered. One patient, a lad, aged 14 years, died suddenly in getting from the bed to the night-stool, apparently from paralysis of the heart, as he was previously doing well and getting about the house; his mother had given him a dose of castor oil contrary to my wishes.

"In respect to the treatment generally pursued, I rely principally on the tincture of sesquichloride of iron, but combine with it the acetate of ammonia and spirits of nitrous ether in order to keep the skin and kidneys active. When there are glandular enlargements, I add to these the iodide of potassium; and should there be also an œdematos condition together with glandular enlargements, both the iodide of potassium and the chlorate of potass. If the tongue be much loaded, and the patient feel at all sick, I generally give a stimulating emetic of equal parts of sulphate of zinc and ipecacuanha; throughout the active stage of the disease an alterative dose of calomel or grey powder combined with the compound ipecacuanha is given every night at bed-time. I apply emollients or stimulating fomentations to the throat externally, and direct the patient to gargle with warm water or weak vinegar and water frequently, unless there be very much fetor. I then substitute a weak solution of chlorinated soda. The forcible removal of membrane and the application of acids or caustics I think quite useless; in fact, I believe they are injurious, producing shock and afterwards greater depression. Perfect rest and quiet are necessary; beef-tea and stimulants, either brandy or wine, as best suited to the case. When indications of approaching paralysis show themselves, I add to the steel mixture small doses of strychnine; thus, I believe, in very many cases obtaining a more rapid convalescence than can be obtained by any other tonic, such as quinine, bark, and the mineral acids, all of which I have tried. Acids I find are not generally tolerated. In some special cases where you have a large amount of semi-fluid fetid discharge from the mouth and nostrils, the solution of chlorine (made by adding dilute hydrochloric acid to chlorate of potass) combined with the steel, and given every two or three hours, I have found to act almost magically in checking this discharge

and producing rapid convalescence. In fact, I find generally that the chlorate of potass with steel to young children is not only the pleasantest, but the best remedy, bringing back the colour to the lips and cheeks sooner than anything else. In cases of croup I give calomel gr.j., ipecac. gr.ij., as a mild emetic every two, three, or four hours, and during the intervals the steel mixture with nitrate of potass and small doses of chloroform, applying externally to the throat and chest fomentations of hot mustard and water, discontinuing the calomel and ipecac. as soon as the urgent symptoms pass off, but continuing the steel mixture. Dr. Paget saw several cases, and amongst them one of these croupal cases. After death I sent the larynx and heart for his inspection.

"Change of air I have found absolutely necessary in some cases; it is beneficial in all. Being situated on the border of the Fens, our atmosphere is of a very depressing character, and those persons who come from a higher elevation and more bracing atmosphere to reside in this neighbourhood generally take the disease.

"On several occasions when there has been a batch of fresh cases those already ill have been taken worse on a particular day and hour and in a particular direction, as if the disease were caused by a wave passing over in this direction from village to village.

"I do not pretend to know or assign the causes or origin of this disease, but I believe with many others that it is a disease affecting the whole system through the medium of the circulation, and always producing affections of an asthenic character; and that when it attacks persons of a tubercular diathesis, if not immediately fatal, it lights up the tubercular disease and hurries rapidly on to a fatal termination cases that probably might otherwise have lived for years."

ART. 18.—*Practical Observations on Diphtheria and Erysipelas.*

By Dr. CHARLES BELL.

(Pamphlet. Churchill : 1864.)

Dr. Bell's principal object in this paper is to recommend muriated tincture of iron in diphtheria. "I was induced," he says, "to employ the muriated tincture of iron in diphtheria by observing its remarkable effects in erysipelas, and being convinced that there is great similarity between the two diseases in many respects, which is evinced by their both being fostered, if not produced by a cachectic state of the system, arising from exposure to an impure atmosphere, and by their being cured by the same mode of treatment." It was Dr. Charles Bell's brother, the late George Hamilton Bell, who introduced into practice the treatment of erysipelas by muriated tincture of iron; and it is the republication of the brother's paper which constitutes what is said about erysipelas in the pamphlet under consideration. It seems to us that the treatment which Dr. Bell recommends in diphtheria is one which will possess little novelty in many quarters.

ART. 19.—*On the Various Forms of Rheumatic Disease in Domestic Animals.*

By M. LEBLANC, Jun.

(Journ. de Méd. et Chir. Prat., August, 1864.)

According to M. Leblanc, three varieties of rheumatism are observed in the horse—viz., muscular rheumatism, acute or chronic; rheumatic synovitis, an almost invariable complication of pericarditis; and articular rheumatism, especially noticeable in the acute form in foals. In the ox, muscular and articular rheumatism are to be met with; in the dog, muscular rheumatism alone has been correctly described; and in swine, acute or chronic mono-articular rheumatism only has been noticed. Sheep appear to enjoy entire immunity from any analogous affection. M. Leblanc's paper was read, during the last session, before the Academy of Medicine at Paris.

ART. 20.—*Diphtheria among Army Horses.*

By Mr. J. H. THOMPSON, Surgeon to the 30th N. York Volunteers.

(Scientific American; and Dub. Med. Press, August 10, 1864.)

A new disease seems to have recently made its appearance in some of the camps of the Federal army in America. Writing about it, Dr. Thompson says:—

“Four horses died in one battery within a few hours of each other. They appeared well in the morning, refused a portion of their oats at mid-day, and in the evening could not swallow anything; rapid exhaustion followed, and in a few hours they died. On opening the animals, the mouth, larynx, and trachea were found covered with diphtheritic membrane somewhat thicker and more tenacious, but in other respects resembling that found in the human subject in cases of diphtheria. If this is diphtheria in the horse, and I know of nothing else it is likely to be, is it with them a new disease, or is it only new to me because I am ignorant of its previous existence? Certainly none of the persons having charge of the Government horses around here know anything of the disease.”

(c) CONCERNING CHRONIC DISEASES.

ART. 21.—*On Scurvy in the Mercantile Marine.*

By Mr. —.

(Lancet, September 10, 1864.)

It is a sad fact that, whereas during the last half century our mercantile marine has increased year by year to an enormous extent,

and opportunities are afforded in every way for promoting comfort and cleanliness on board ship, the condition of that vast floating population on which our commercial prosperity depends has, as to its sanitary prospects, undergone no change whatever. We build model lodging-houses for the labouring poor of our towns; we appoint inspectors of nuisances for our streets and by-ways; we spend thousands, and rightly too, in draining this great metropolis by a monster sewerage-system; but we will still persist, as to sanitary matters, in forgetting the very existence of the sailor. He still subsists, as in years gone by, on his ration of salt pork and hard biscuit, and, in very many instances, is still penned up in a close forecastle ingeniously constructed, as it would appear, to let in as little light and as much water as possible. This sleeping-place, cleaned as seldom as the skins of its inhabitants, is by no means as sweet, towards the close of a voyage, as could be desired. Indifferent water with bad meat and biscuit are often the only food of the sailor for three or four months together; and it is well known habitual indifference to personal cleanliness is a powerful predisposing cause of disease. To crown these evils, and as though to give every opportunity for an outbreak of scurvy, we learn from reliable sources that a large portion of the lime-juice shipped for use in our India and China vessels is completely spurious. Quoting a letter headed "Spurious Lime-juice" that appeared in *The Times* of August 29th from a Liverpool correspondent, we are informed that

"A large quantity of so-called lemon-juice which is supplied to ships going on long voyages contains no juice of the lemon; it is manufactured in this country from tartaric and other acids, at a cheap cost, and flavoured with essence of lemon to imitate the genuine article. I have had many samples analysed, and found them to be so made up."

This total neglect of all sanitary precautions in our merchant navy culminates in the following unhappy results. During the past year the *Dreadnought* received eighty-six cases of scurvy; and we are informed by the medical officers of that hospital that this represents the average annual number of entries during the last ten years, no diminution as to yearly admissions having taken place during that time. Fifty cases were admitted into the Liverpool hospitals collectively, and a few cases found their way into our metropolitan hospitals. The report to the Privy Council also states that "of the entire number of men received into the Sailors' Home at Poplar, about half are at the time of their admission suffering more or less from scurvy, and of these perhaps a twentieth part are seriously diseased."

The above statistical facts reveal an existing amount of preventible disease perfectly appalling. These show, however, very inadequately the other ills indirectly caused or greatly aggravated by scurvy; for the *Dreadnought* records could furnish us with many instances in which sailors have succumbed to acute attacks of other maladies, solely from the excessive debility that scurvy has left behind. It is for our commercial interests, as well as on the

score of humanity, that this blot should be removed from our national shortcomings by legislative means. The exertions of many charitable individuals have been nobly displayed in founding for the sailor clean and comfortable "homes" during his brief sojourn in this big city, and these praiseworthy efforts have been crowned with very great success. But the attention of our Government should be seriously directed to the improvement and good maintenance of the seaman's sanitary condition when afloat; and he has, with the agricultural labourer and the manufacturing artisan, an equal claim to its exertions. Underwriters and owners would benefit alike: for there would be less risk to the former of loss of vessel and cargo from want of manual power in case of leakage or other accidents; and there would be a more certain prospect to the latter of speedier voyages, and of having in the sailor commodity a subject not, at all events, broken down by, and patched up after, one of the most terribly exhausting diseases of which we are cognisant, and one too that ought many years ago to have been struck out of our sick returns.

ART. 22.—*On the Employment of Benzine in Trichiniasis.*

By Dr. MOSLER.

(*Berliner Klin. Wochenschrift*, 32, 1864; and *Edin. Med. Journal*, September, 1864.)

In his late work on intestinal worms (*Helminthologische Studien und Beobachtungen*, Berlin, 1864), Professor Mosler directed attention to the poisonous influence which benzine exerts upon trichines in the intestinal canal, and he further narrated an experiment which seemed to show that the trichines in the muscles did not perhaps altogether escape from the action of this substance. A pig which had been infected with trichines was treated during four weeks with increasing doses of benzine. It then died. The trichines with which its muscles were crowded had a peculiar appearance, and showed no movements even when the temperature of the flap in which they were placed was raised. A rabbit fed by Dr. Mosler with the flesh of the animal died in eight days, and only presented intestinal trichines in very small number. Dr. Leuckart sent a certain quantity of the same flesh to three of his friends, who administered it to a series of rabbits. The experiment failed completely in all these animals except one, in which, after great trouble, a single trichina was found. It seemed from these results that the benzine had killed the immense majority of the trichines in the muscles of the pig. But it was believed that the pig had been poisoned by the benzine, and hence it was not to be expected that this substance could be used successfully to combat trichiniasis in man. Later experiments have, however, proved to Dr. Mosler, that larger doses than he had given in this case can be supported with impunity; the death of the pig had been caused by a pneumonia, which had been occasioned by the accidental penetration of benzine

into the air-passages, the result of a faulty mode of administration. It was thought a very interesting question to determine if benzine in large doses could be borne by the human economy in the febrile condition which exists at the commencement of trichiniasis. Dr. Mosler had an opportunity of studying the question on a large scale during the alarming epidemic of trichiniasis which desolated Quedlinburg at the beginning of the present year. He went there on the 20th of March, and the treatment by benzine was immediately put in force by himself and the resident physicians. At first it was given in capsules of gelatine, but this method was soon given up as expensive and troublesome. The following mixture was usually given : benzine, two drachms ; liquorice juice, mucilage of gum-arabic, of each an ounce ; peppermint water, four ounces. A tablespoonful every one or two hours, the bottle having first been well shaken. In this form benzine was easily taken and well borne. Many of the patients stated that under this treatment they soon felt better, and that the pain in the limbs was relieved. The largest doses were given by Dr. Rudolff, who gave as much as two drachms daily, and this was continued for from four to six days. Dr. Mosler had the opportunity of satisfying himself that in none of the patients, even in those in whom the fever was high, did any bad effects show themselves ; in particular, there was no appearance of nervous or pulmonary affections, which are so often observed when benzine is administered to rabbits. Dr. Mosler undertook a fresh series of comparative experiments upon pigs, putting himself as far as possible in the same position as a physician would be, both as to the doses of the remedy and as to the time when the treatment was commenced ; that is to say, commencing a week after the injection of the trichinæ, a period at which trichiniasis can be diagnosed in man. The following are the conclusions to which Dr. Mosler has been led by his observations and experiments :—That benzine, which holds the first place among all the anthelmintic remedies, may be administered to man without bad effect in larger doses than was formerly thought possible : That, given in doses which the human organism can tolerate perfectly, it kills with certainty trichinæ in the intestinal canal, and so prevents the emigration of the embryos ; and that therefore benzine is the only rational treatment to be employed in the trichina disease of man.

SECT. II.—SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 23.—*A Case of Meningitis of the Base of the Encephalon.*

By DR. W. DERBLICH.

(*Spitol's Zeitung*, July 30, 1864 ; and *Brit. Med. Journal*, Sept. 17, 1864.)

CASE.—A. G.—, a soldier, twenty-four years of age, a large made robust man, was brought into hospital on July 5th, 1864. He was pre-

feetly conscious, and complained of universal tenderness, especially in the head, right knee, and back. He had always enjoyed good health, until nine days before admission, when he returned home heated after exercise, hastily drank a pint of water, and afterwards felt oppression in the chest, and in the course of the day had alternations of heat and chills. Slight febrile rigors, loss of appetite, and sleeplessness, were followed by return of appetite, loss of pain, and disappearance of fever. This was within the first four days; and on the fifth day he was sent on foot as sentry to a station some miles distant. He arrived wet through by rain, was unable to obtain any rest during twenty-four hours, and on the next day was seized with violent pains in the limbs, loins, and head. These increased so greatly, that, when admitted into hospital, he could not turn his head nor draw up his feet towards the abdomen. The temperature of the whole body was remarkably increased; the face was red; the conjunctivæ were somewhat injected; the pupils were not dilated; the countenance was clear; there was no photophobia; the tongue was covered with a white fur, and could be moved in any direction, but trembled slightly. The head was strongly turned to the left; and any attempt to move it to the right caused much pain. Touching the nape of the neck caused pain, although nothing abnormal could be detected either here or in the muscles of the neck generally. The carotids on both sides pulsated vigorously. The chest was strongly arched; the respirations quickened—42 in a minute. The percussion-sound was everywhere clear and full. By immediate auscultation, an exaggerated, slightly rough breath-sound was detected. The heart was in its normal position; its impulse was strong and full; the first sound was accompanied by a scarcely audible blowing murmur. The abdomen was somewhat enlarged, but the percussion-sound was duller than normal. The stomach was moderately full. Nothing abnormal could be detected in the liver, spleen, or intestines; the bladder was moderately full of urine. The patient was in a state of general depression and *malaise*; he was rather feverish (pulse 78), and complained of entire loss of appetite and moderate thirst. His bowels had been open in the morning; the evacuation was loose, and was unattended with tenesmus. Eight leeches were applied behind the ears, and ice to the forehead; and one-sixth of a grain of morphia was prescribed. Lemonade was ordered for drink.

The next day, July 6th, the patient was found in the morning to be in nearly the same condition. He had slept but little, and had been very restless. His consciousness was still undisturbed; but he was more excitable. His manner of expressing himself was quicker, and his gestures were vehement. He would rise up, but could not raise his head. Motion of the head to the right was impossible on account of pain. There was slight divergent strabismus in the right eye. The tongue was moist, and moderately covered with white fur; it could be protruded in all directions. The breathing was somewhat accelerated, but otherwise normal. The heart's action was somewhat stronger than on the previous day; the blowing murmur was heard during the diastole. More than two pints of reddish urine, of high temperature, and rich in urates, were drawn off by the catheter. The right knee was very painful; but there was neither redness nor swelling of the part. He had no appetite; was thirsty; and his bowels were constipated.

The continued pain in the head, the rigidity of the left sterno-mastoid muscle, the inability to hold up the head, the extreme tenderness of the nape of the neck, the strabismus, the paralysis of the bladder, and the hypersæthesia of the right lower limb, pointed to pressure on the brain by a limited exudation. In order to moderate the hyperæmia and promote absorption, leeches were ordered to be applied, and the ice to the forehead

was repeated ; and the patient was directed to take every three hours a table-spoonful of a mixture containing half a drachm of iodide of potassium, and half a grain of morphia, in four ounces of water. Cupping-glasses were also applied to the back, and sinapisms to the calves of the legs. Towards noon, convulsions appeared ; the patient had slight trismus, and drank with difficulty. The face was very red ; the right pupil was dilated and distorted. The respiration was difficult ; the heart's stroke quicker and weaker ; the pulse was small and very rapid ; consciousness was much obscured. In the evening, perfect sopor set in ; the breathing became stertorous. Blood-letting, cold affusion, and clysters were employed ; and the urine was drawn off by the catheter. The patient was afterwards found in a state of stertor, and unconscious ; but, when attempts were made to turn the head to the right, clonic contractions of the muscles of the face took place. The pupils acted but slightly under the influence of light ; both eyes were strongly injected ; the lips were nearly dry ; the respiration was frequent and irregular ; the pulse less frequent. The lower limbs were cold to the touch, and were entirely painless on deep pressure. The bladder was much distended. The urine was drawn off by the catheter ; and a yellow fluid stool was passed involuntarily. Towards midnight, the stertor increased ; the patient had more violent contractions of the muscles of the nape of the neck and general twitchings ; the redness of the face disappeared ; the forehead and face became covered with a clammy sweat ; and the patient died at two A.M. on July 7th.

A post-mortem examination was made thirty-six hours after death. There were some brownish-red post-mortem ecchymoses on the surface of the body. The expression of the countenance was quiet ; the eyelids were sunken. Both pupils were dilated, the right more than the left. The lips were closed, and covered with dry mucus. The abdomen was somewhat distended, and presented several bluish-green spots, where decomposition was commencing. On sawing through the skull, the bones were found to be very thick ; the diploë especially was much developed, and very vascular. In the neighbourhood of the sutures, however, the bones were thin and remarkably transparent. The dura mater was firm and vascular ; as were also the Pacchionian bodies, which were enlarged. The arachnoid could be separated at parts from the dura mater ; on the convexity of the brain especially, it appeared macerated. The vessels of the pia mater were strongly injected ; and in some of the convolutions it was infiltrated with a muddy serum. The lateral ventricles were dilated, especially the right, and contained about an ounce of whitish tolerably clear serum. The brain-substance did not present any considerable amount of hyperæmia. The pons Varolii and the medulla oblongata, with the nerves issuing from them, were covered with a greenish purulent exudation. In the three fossæ of the base of the skull there was a large quantity of reddish serum mixed with greenish pus. The cerebellum showed many vascular points on section ; the medullary substance was firm. No pathological appearances could be found in the muscles of the nape and left side of the neck. The pleuræ were adherent posteriorly ; the lower lobes of the lungs were slightly edematous ; the bronchial tubes were partly filled with reddish mucus. The pericardium contained a little clear fluid. There was a considerable deposit of fat on the left side of the heart. The right side of the heart was much dilated, and was distended with dark-red clotted blood. Nothing remarkable was found in the abdominal organs ; there was no trace of tubercle in any part.

AET. 24.—*On Pain in the Knee-Joint as a Symptom of Meningitis.*

By Dr. LUND.

(*Dublin Medical Press*, June 29, 1864.)

At a meeting of the Norwegian Medical Society, held on the 25th of November, 1863, Dr. Lund said:—

"I wish to mention a phenomenon which I have sometimes observed in children suffering from meningitis, and which I look upon as bearing on the diagnosis of this disease. I have, in fact, found that, either before the development of the proper cerebral symptoms, or after the latter have appeared, a pain, which I shall just now more fully describe, may set in in one or both knee-joints. As I have neither in manuals nor in journals seen this pain in the knee given as a symptom of meningitis, and as I have not heard it mentioned by my colleagues, I feel called upon briefly to state the cases in which I have made this observation.

"The first time my attention was drawn to this symptom was sixteen or seventeen years ago, when a girl, aged about nine years, who, after supposed exposure to cold, was attacked with violent pain in the right knee-joint, on examination of which nothing could be discovered, came under my care. The pain was increased on any attempt to move the joint, there was no pain in the hip-joint or in any other part of the extremity, but there was some fever and general disturbance. A brother of the patient had some time previously died of inflammation of the brain. As I have no notes of this first case, in which I observed pain in the knee as a symptom of meningitis, I can state only that signs of cerebritis, which soon ended fatally, set in first several days after the occurrence of the pain.

"The second case occurred in February, 1858, in a girl, aged 14, who, in proportion to her age, was rather tall, but slight, some time before had suffered from spasms resembling chorea, and had over-worked herself at reading. She suffered for a couple of days from fever, headache, delirium, vomiting, and constipation, and was then attacked with violent pain in both knee-joints. The pains were so violent and persistent that she was constantly moaning and crying. She could bear neither movement nor change of posture of the extremities. Neither swelling nor any change in the colour of the skin was discoverable in the joints. There was no pain in the hips or in any other part of the extremities, nor was there any pain in the back. She was treated with leeching to the temples, sinapisms, aperients, cooling mixture, calomel, &c. The pains in the knees continued, notwithstanding the employment of various local means, unchanged for more than two days and nights, after which time a sudden aggravation of her general symptoms occurred, violent spasms retracting the head and bending the whole trunk in the same direction, and constant delirium made it difficult to decide whether the pains in the knees continued or not. She had quina

and a warm bath without producing any alleviation of the spasms. Picking of the bedclothes, subsultus tendinum, and cyanosis supervened, and death occurred on the ninth day from the commencement of the illness. No post-mortem examination could be obtained. Professor Voss assisted me in the treatment of this patient.

"In November of the following year the third case came under my care; it was that of a boy, aged six years, who was labouring under pulmonary tuberculosis. His mother suffered from the same disease. I was called in in consequence of his having been attacked with violent pain in the right knee-joint. Dr. Skjelderup and I, who together treated this boy, could not discover anything in the form or appearance of the knee capable of explaining the pain, which was increased on movement of the limb, and persistent. Some time after, the boy had pain in the right hip, leading us to consider the possibility of commencing inflammation in that joint, on which the pain in the knee might be dependent. But about a month after the commencement of the pain in the knee, and after various remedies employed during that time to relieve the pain had been used in vain, symptoms of tubercular meningitis, of which he died, set in. When the cerebral symptoms became developed, the disturbance of his intelligence prevented us from deciding whether the pain in the knee continued. There was no post-mortem examination.

"The fourth patient in whom I have observed pain in the knee as a symptom of cerebritis, has been under my care during the present year. A boy, aged six, who was in tolerably good condition, and had not, like his brothers and sisters, suffered from cutaneous eruption, had in the beginning of February an attack of angina parotidea, attended with the usual symptoms, and subsequently suffered from slight bronchitis and urticaria. Some time after he lost his usual vivacity and inclination to play, and became dull and peevish. At the end of March he was attacked with a rather violent pain in the left knee-joint, where, on examination, nothing could be discovered. He kept the limb constantly extended, could not rest upon the leg, and any attempt to bend the knee or in any way to change his position in bed increased the pain. He suffered from no other pain, either in the hip or elsewhere. His general health appeared to be very good, still he continued to be peevish. The knee was rubbed with anodyne liniments, and was kept warmly covered, but without producing any apparent effect upon the pain. After the lapse of about three weeks the pain ceased, and the patient began to go about the room, and even went out sometimes, played with his brothers and sisters, and seemed to be more lively than he had been for the last two months. On the 28th of April he had slight fever with determination to the head, lost his appetite, vomited occasionally, and was constipated; he again became dull, lethargic, and cross; the pulse was not quick. Cooling mixture, aperients, enemas, sinapisms, produced no change in his state. He became more drowsy, did not answer when spoken to, asked for neither food nor drink, but when drink was brought to his mouth, he took it eagerly. For a couple of days there was reason to suspect the existence of incipient typhus; but as there was no abdominal tender-

ness, as no maculae were visible, as the tongue continued moist and the cardiac sounds remained unenfeebled, this diagnosis was given up, especially as more distinct signs of a cerebral affection set in—namely, ptosis of the left upper eyelid, dilated pupils, contraction of the right arm, and spasmodic startings and tumours in the upper extremities, rigidity and some opisthotonus of the body, inclination of the head to the left side, deep inspirations combined with sighing, &c. In addition to aperients, quina was given internally, and he had repeated blisters to the nape of the neck and behind the ears. The pulse, which at first was of normal frequency, became slow, between 50 and 60 in the minute, and subsequently hurried and irregular. Deglutition was difficult, the urine passed involuntarily, coma and death supervened on the 11th of May. Dr. Gottfr. Conradi, who had treated the patient with me during his whole illness, also assisted me at the post-mortem examination, which took place sixteen hours after death. We were allowed to open only the skull. The arachnoid and pia mater were everywhere congested with blood. Along the course of the vessels between the membranes there was serous exudation, which in some places was turbid, and was of a light yellow colour. There were some small white points in the membranes, especially in the neighbourhood of the longitudinal sinus. At the base of the brain, at the optic commissure, there was, between the membranes, a green purulent exudation of tolerably great extent. Within the circumference of this the arachnoid was studded with many slightly prominent, groat-like, white tubercles. Both lateral ventricles were distended with watery serum.

"As I have already said, I have not seen nor heard the kind of pain in the knee described in these cases mentioned by any one as a symptom which may accompany meningitis in children. This appears to me the stranger, as I have observed the symptom four times. Omitting the first case, the other three refer to a space of ten years, in which I have notes of the cases of meningitis in children treated by me. As during that time I have had thirty such cases under my care, this symptom, if it occurred as often in the practice of other physicians, would be present in one case in ten—that is, in ten per cent. But the number I have met with is too small to justify me in venturing to draw any conclusion as to the frequency of the occurrence of the symptom; but I doubt not that my colleagues, if they direct their attention to the point, will soon find cases of meningitis in which it is present.

"Were I, from the few observations I have made, to draw any conclusions as to the symptom in question, they would be the following:—

"Pain in the knee occurs not very unfrequently as a symptom in meningitis in children. It is met with in about ten per cent. of such cases.

"This pain in the knee is persistent, is sometimes very violent, increases on every movement of the joint, and is not relieved by local means. It is connected neither with swelling nor with any other change in the form or appearance of the joint.

"It may attack one or both knees.

"It may occur some time before the distinct development of the other symptoms of cerebral inflammation, and may then either (as in the fourth case) cease before the appearance of the latter, or it may continue (as in the third case) until the disturbance of intelligence makes it uncertain whether it any longer exists; or it may (as in the second case) manifest itself after the cerebral symptoms have already been developed.

"It may present itself both in acute and in chronic cases. It seems to be most violent in the more acute cases, and in those combined with severe spasms (as in the second case.)

"In the cases observed by me it occurred between the sixth and the fourteenth year, and in both sexes.

"I am not aware that I have observed a pain in the knee in children presenting the characters I have described, except in connexion with meningitis. Therefore, I assume that when it occurs, it must be regarded as a not unimportant diagnostic sign. Either it will, by its appearance before the development of the cerebral symptoms, give a hint that meningitis is to be expected, or by manifesting itself in the course of that disease, it may afford a distinctive mark, facilitating the diagnosis between meningitis and other affections liable to be confounded with it, as, for example, typhus.

"I will not attempt any further explanation of the occurrence of this symptom, especially as I have had the opportunity of examining post-mortem only one of the four cases which were under my care, and in that one I had permission only to open the head. That all four patients laboured under cerebritis, must be looked upon as certain. But whether the pain in the knee in these cases was connected with and was caused by the changes existing in the brain, or whether it should be ascribed to a coexistent inflammation in the spinal marrow or its membranes, is perhaps more difficult to decide. But in favour of the first view I may remark, that the pains in the lower extremities which accompany inflammation of the spinal cord, usually have a greater extent, and are not confined to particular joints, are most frequently combined with spasmoidic twitchings in the extremities, and soon pass into paraplegia, not to mention that the dorsal pain in this affection is generally tolerably well marked."

ART. 25.—*Right Hemiplegia and Aphasia, without any Lesion of the Third Left Frontal Convolution.*

By Dr. EM. FARGE, Professor of Clinical Medicine at the School of Angers.

(*Gazette Hebdomadaire de Médecine et de Chirurgie*, No. 44, Oct. 1864.)

CASE.—François Chaslon, a weaver, aged sixty-one, is admitted into the Hôtel Dieu of Angers, June the 8th, 1864. Two days previously he had suddenly lost consciousness, and fallen down completely paralysed on the right side. He is brought to the hospital in the same state, and the pulse being found full and frequent, leeches behind the ears, sinapisms, &c., are ordered. On the following morning he seems still unconscious of everything

around him, with eyes fixed, and answering no questions. The left angle of the mouth is pulled up a little, and there is some slight distortion of the left side of the face. The right upper and lower extremities are both perfectly flaccid, their sensibility very obtuse, and their motility completely gone. The skin looks dull, somewhat discoloured. There is some oedema of the feet and legs, which may be accounted for by the condition of the heart, which intermits at about every thirtieth beat; and there is besides well-marked jugular pulsation. The urine comes away in drops, although there be no distension of the bladder. There has been no action of the bowels. The treatment consists in derivation to the skin and intestinal tube.

June 10th.—Several involuntary stools. Pulse small, soft, frequent, still intermitting a little. Very doubtful consciousness, although the patient seems to move his left arm when called: the movements are more manifest when he is pinched.—June 11th. Hemiplegia as before, with, however, a slight increase in the sensibility of the right arm. There is partial return of consciousness, for he gives his left hand, or raises it to his head, according as he is bid; and if asked for his right or palsied hand, he goes in search of it with his left. To questions put several times he invariably answers, "Ah, si! Ah, oui!" whatever be the question asked; and if pinched, he cries out the same words.—June 13. He has less difficulty in answering, and does so on the second and even the first time of asking the question. If he be pricked, he now exclaims, "Ah! bon sens de Dieu!" and any painful impression is sure to bring out the same exclamation. He still moves with intelligence, and according as he is bid, his left arm, his tongue, his lips, &c. Deglutition is more easily performed, and when he drinks none of the liquid now runs out of his lips. On the 14th, when told to do so, he attempts to repeat the words: "Tisserand, medecin, bon jour," after they have been several times uttered in his presence. He sometimes succeeds, often leaves out a syllable, and oftener still replaces one of the words by "Ah, si! . . . Ah! bon sens de Dieu! . . ."

From the 14th to the 17th, he seems to progress favourably, moves the fingers of his right hand, and next his right arm and shoulder, and also succeeds in partially flexing his right leg. Intellect better. He answers in the affirmative or in the negative, according to the question put, and without making any mistakes. Fatigue or any cause of annoyance, however, makes him invariably call out, "Ah! bon sens de Dieu!" He can now repeat whole sentences after four or five failures; but the act seems to be purely automatic, and he adds to the sentence, "Ah, si! Ah, oui!" Phonation and pronunciation are perfect; and, with the exception of a little thickness, manifestly due to an accumulation of mucus in the fauces, his articulation may be said to be very clear and distinct. The same treatment is continued as previously, with the addition of some tincture of nux vomica.

Up to the 27th no change occurs, but from that day he becomes dull, answers badly and less frequently, and will not repeat words as told. Cough and dyspnoea set in.—June 28th. Dulness of aspect increases; the dyspnoea also.—29th. Fever, prostration, confirmed pneumonia; patient does not answer questions at all, grows more and more dull, and the symptoms gradually getting worse. He dies on the following evening.

At the post-mortem examination, made July 2nd, in the morning, the following appearances are noted down:—Right pleuro-pneumonia. No appreciable organic disease of the heart. Convex surface of brain normal; by making horizontal sections of the organ, the left centrum ovale of Vieussens is found softened over an area of the size of a small egg: the outer two-thirds of this area are white, but the inner one-third is of a reddish colour, and consists of small coagula, nearly discoloured, and an elongated sinuous mass, of harder consistence, and looking like an old cicatrix. (All

the arteries distributed to the left hemisphere, as far down as the carotid and the vertebral, are free from any embolism.) The softening extends no deeper than the roof of the lateral ventricle, although the superficial portion of the optic thalamus has something of a gelatinous, softened look. Corpus striatum normal. Viewed at the base of the brain, the anterior and middle lobes of the left hemisphere look as free from disease as those of the right. The margins of the fissure of Sylvius, the insula of Reil, the marginal and longitudinal convolutions, more particularly the third frontal convolution, are carefully examined on both sides. No difference of volume, aspect, or consistence is perceptible to the unaided eye, or with the help of a magnifying glass powerful enough to show the lymphatics. Thin slices of the cerebral substance on the left side are examined microscopically, and exhibit no transformation of the nerve-tubes, no alteration of their shape, no interruption in their continuity. True, the cylinder axis cannot be seen, but it must be remembered that the examination is made in July, at least thirty-six hours after death. A few oil-globules are seen here and there, but in very small numbers; and there is no suspicion of the presence of a single pus-cell. The capillaries are very apparent, and although they contain a few altered cells, there is no degeneration of their coats. Slices of the cerebral tissue of the right hemisphere, similarly examined, present exactly the same appearances. The ventricles contain some faintly-yellowish limpid serosity, amounting in quantity to about the third part of a tumbler. The septum lucidum is very distended; the walls of the ventricles are pushed out, but not distended, and they are equally so on both sides. There is no atrophy, no alteration of the pons and medulla oblongata.

In the accompanying remarks upon this case, the author draws attention to the results of the post-mortem examination, which are in direct opposition to the theory of a relation existing between loss of articulate language and disease of the third left frontal convolution. He endeavoured, he says, to fulfil all the conditions imposed by Dr. Aubertin, in the *Gazette Hebdomadaire* (1863, Nos. 20 to 25). He confesses to have rapidly and only incompletely examined the coordinating apparatus to which Dr. Jacobson has lately drawn attention again, and especially the olivary bodies. More careful search might, perhaps, have detected some lesion in the latter; for the patient had difficulty in swallowing in the beginning. But the aphasia could not have been produced by any such lesion, if it existed; for the dysphagia disappeared when the absolute loss of speech was succeeded by dysphasia. Besides, the patient did not lose his power of speech from any alteration in the coordinating apparatus on which articulate language depends; for the words which he uttered spontaneously were very distinctly pronounced, and comprised various combinations of sounds, whilst those which he repeated automatically might indeed remain incomplete and unfinished, and might not correspond to an intellectual act; but the larynx, the tongue and the lips concurred in their production with harmonious coordination. Nor can it be objected that the case is not one of aphasia; for, if at first the loss of speech may be considered as an unavoidable and very secondary consequence of the loss of consciousness, we see consciousness first returning as shown by the patient hearing, performing the movements he is told, whilst speech becomes possible only a little later. For three days,

the stock of language merely consists of two words and a portion of a sentence, which are repeated on every occasion, and applied to everything. Next, in a third stage, words uttered several times in the patient's presence and hearing are repeated by him, although a few only at a time, without a fixed purpose, or a clear intellectual act, after the manner of a child who, when asked to say papa or mamma, either remains silent or utters the word he is not asked for. Lastly, all words cease with the setting in of the coma, which was doubtless due to the intra-ventricular effusion.

On these grounds, the author concludes that his case is in opposition to the theory which absolutely maintains a relation to exist between aphasia and disease or injury of the third left frontal convolution, and that it favours the doctrine of localization in one particular only, viz., the exclusive alteration of the left hemisphere. The present case is like the one recorded by Dr. Charcot, and increases the proportion of cases in which aphasia may be considered not a disease but a symptom. Lastly, it tends to prove that this symptom may be due to variable lesions of the brain, provided these be sufficiently extensive and grave to influence the system at large. Disorganization of the whole centrum ovale of one cerebral hemisphere is doubtless often met with unaccompanied with aphasia; but when the intellect is deeply impaired by it, can it be considered strange that the verbal expression of the intellect should be affected in particular?

The editor of the *Gazette*, Dr. Dechambre, further remarks that the above case belongs to the third variety of aphasia admitted by Dr. Jaccond, in his recent essay on the subject, viz.—the one characterized by an interruption in voluntary transmissions. The case is besides complicated. The intellect was impaired, as shown by the symptoms during the first few days, and by the phenomenon of echo (Romberg), temporarily noticed. The few words that could be uttered were normally articulated, and this is a diagnostic sign given by Dr. Jaccond, between aphasia due to a defect of co-ordination and aphasia due to a defect in voluntary transmissions, in those cases in which the loss of speech is not absolute. The olivary system, therefore, could not have been diseased. Lastly, as in many of the cases belonging to the third variety, the loss of speech was not complete; the lesion was unilateral only, whilst up to the present time complete dumbness has only been met with in cases of bilateral disease.

ART. 26.—*On the Pathological Anatomy of Tetanus.*

By Mr. J. LOCKHART CLARKE.

(*Lancet*, September 3, 1864.)

After a brief summary of what is at present known of the pathological anatomy of tetanus, Mr. Clarke gives the results of some observations which he lately made in the two following cases:—

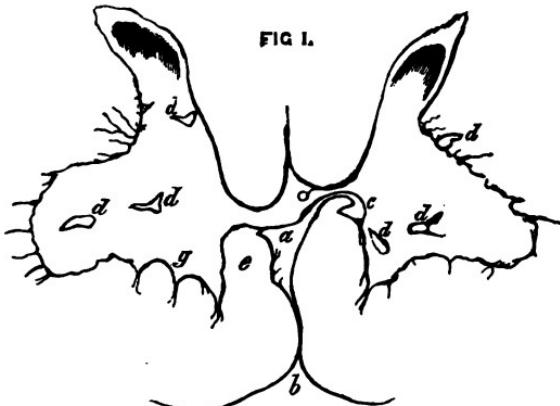
" On the 30th of April, 1863, a man fell a distance of twelve feet
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to the ground, by which the skin of the right leg was said to be severely bruised. On the 11th of May he complained of stiff neck, and some difficulty of deglutition. In the night his mouth became quite closed, and he had several fits of severe dyspnoea, accompanied by contraction of the muscles of the back. On the following day the surface of the body became cold and clammy, with large drops of sweat on his forehead. When lying supine on the bed, his back was so curved that the hand and forearm could be passed between it and the bed. He had the *risus sardonicus* expression of face. The pulse was weak, but regular—100. Every ten or fifteen minutes there was momentary contraction of the facial and spinal muscles. Attempts at deglutition, or at taking a deep inspiration, brought on these spasms immediately. The breathing was nearly all abdominal, the thorax moving very slightly. The right leg was inflamed about its middle, and there was a deep slough four inches long and two inches wide. The actual cautery was applied from the occiput to the middle of the dorsal region, the patient being under the influence of chloroform. The spasmoid attacks gradually increased in frequency as well as intensity, and the man died in one of them on the morning of the 13th. He could move his jaw freely during the last four hours.

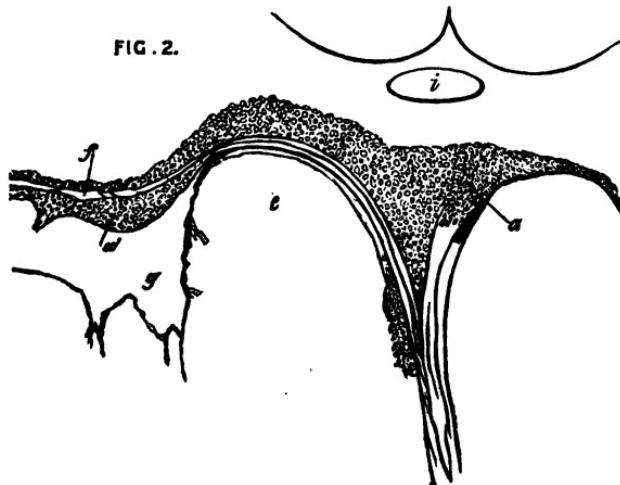
"The post-mortem examination was conducted by my friend Dr. Dickinson, of St. George's Hospital, who gave the following account of the external aspect of the spinal cord:—'The sheath of the cord was natural in appearance. It had a reddish hue, owing to the fulness of the vessels on the surface of the cord. Over its whole extent the cord was covered with large injected vessels, which were nearly as thick as whipecord. They were near together, and ran more or less parallel to the length of the cord. The white and grey matter were both congested, and the puncta were very conspicuous everywhere.'

"A portion of this cord—the principal part of the cervical enlargement—was given to me by Dr. Dickinson. It was already hardened by long maceration in chromic acid. In sections I found the grey substance in particular very much congested; and not only were the vessels unnaturally dilated, but each was more or less surrounded by a granular and originally fluid exudation, in which the natural tissue of the part became broken down and ultimately dissolved. In Fig. 1, a large triangular mass of this exudation is represented at *a*, where it occupies the bottom of the anterior median fissure (*b*), and has destroyed a part of the anterior commissure by extending to the right as far as *c*. The same parts of the grey substance of other sections, more highly magnified, are represented in Figs. 2 and 3. In Fig. 2, a large quantity of granular exudation (*a*) has enveloped and partially destroyed some blood-vessels, and the pia mater which supports them. The exudation extends to the left, around the bottom of the anterior column (*e*), destroying a portion of the anterior commissure, and following the course of an evidently diseased blood-vessel (*f*) into the middle of the anterior cornu (*g*), where it has destroyed a part of the grey substance. In Fig. 3,

(p. 52), we find at *a* the same kind of granular exudation enveloping the pia mater and blood-vessels which enter at the anterior median fissure; and at *b*, in the same figure, is a separate and oval mass of the same kind of exudation, surrounding the cut end of one of the blood-vessels as it bends round on the right side of the canal. On

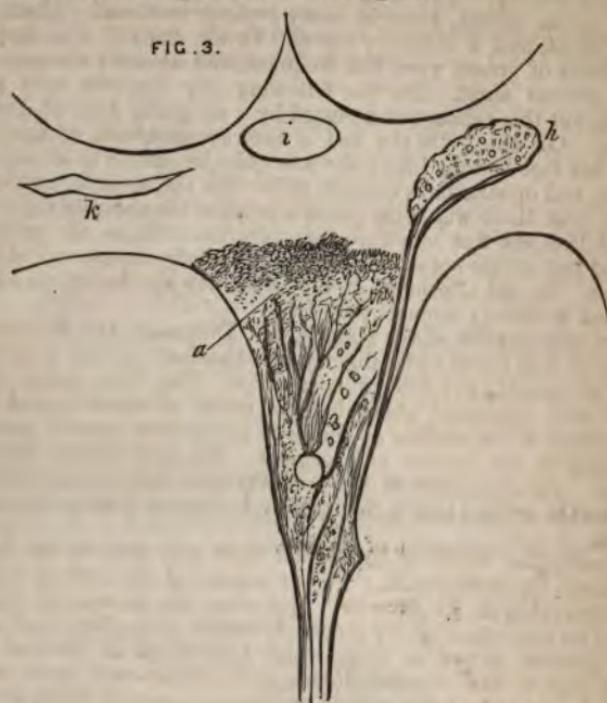


the left side of the canal, at *k*, is another elongated, finely granular, and almost pellucid area of degeneration. On turning to Fig. 1,



we find that small areas or patches (*d*, *d*, *d*, *d*, *d*), which under a high power are seen to be of this description, are scattered over different parts of the grey substance. Most of them may be seen to lie, as they probably all do, at the side of or around blood-vessels.

In some places they are exceedingly minute, and much more numerous than they are represented in the figure.



"Of the second case of traumatic tetanus the history is as follows:—

"On December 26th, 1863, a girl, aged seven years, trod on an iron meat-stand, and received a small wound on the ball of the toe of the right foot. Some pain and inflammation followed, and the part was kept poulticed. On Jan. 7th, 1864, her mother noticed a peculiar expression about the face, and forty-eight hours later the girl complained of pain about the lower jaw. On the morning of the 10th she had lost all power of opening the mouth. On the evening of the same day her head began to be drawn backward, and her spine to be somewhat curved. The following day she had opisthotonus and sardonic grin; the pulse was small and quick. The wound in the foot had healed, and the cicatrix was not painful. The posterior tibial nerve was divided by Mr. Henry Lee. On the 12th the risus sardonicus was less marked; she could open her mouth about half an inch, and protrude her tongue easily; deglutition was perfect. Any sudden movement of the bed-clothes, however, threw her into a state of spasm, in which the back became bent, and the corners of her mouth much drawn down. The abdomen was hard;

respiration 38 per minute; pulse 160 and regular; she answered questions readily. Two doses (each three grains) of calomel, followed by an enema, brought away copious motions. There was great thirst, and a constant cry of 'Drink, drink!' On the 14th the attacks of spasm were less frequent and severe; she passed a much quieter night. On the following day she was very much purged, but this action was arrested by a six-grain dose of Dover's powder. On the 16th she had a very severe attack of spasm, in which her face became blue; she was quite insensible to all external objects, and opisthotonus ensued. After this she slept for two hours. On the 18th there was some pleurostethonos towards the right side. On the 19th she was much weaker. On the 20th, in the morning, she had two severe spasmodic attacks, with only an interval of a few minutes. In the afternoon of the same day she had three others, and died at twenty minutes past two.

"On examination of the body by Dr. Dickinson, the pia mater of the brain was found to be slightly blood-stained in the neighbourhood of vessels, but otherwise healthy. The dura mater of the spinal cord contained about half an ounce of blood-stained fluid. The vessels of the surface of the cord itself were greatly injected, especially in the lumbar enlargement. Its grey matter was congested. The remainder of the cord was more than usually vascular, both on the surface and in the interior, but less so than in the lumbar region.

"Nearly the whole of the spinal cord was sent to me by Dr. Dickinson for examination. To the unassisted eye nothing unusual was observable in its interior. But when the microscope was employed on sections properly prepared, lesions were discovered of the same general nature as those which I described in the first case. Every region—the cervical, dorsal, and lumbar—was more or less affected; but in each the morbid appearances seemed rather to occur at intervals, and not uniformly throughout its length. In some sections the injury was limited to the grey substance; in others it involved the white columns, particularly the posterior and lateral. Sometimes the lesion was in the form of a granular deposit around blood-vessels; sometimes in the form of globular masses or rings, arising from injury and displacement of the white substance of the adjacent nerve-fibres. The latter appearance was more frequent in the white columns and along the sides of fissures containing blood-vessels, where the tissue had occasionally the aspect of a moth-eaten cloth. In some places the lesional spots were exceedingly small, and might easily have been mistaken by an unpractised eye for the natural appearance of the part."

ART. 27.—*On Ganglionic Epilepsy.*

By Dr. RAMSKILL, Physician to the National Hospital for Paralysis and Epilepsy.

(*Medical Times and Gazette*, May 28, 1864.)

The case quoted below, and the accompanying remarks, form the substance of a clinical lecture delivered a short time ago at the National Hospital for Paralysis and Epilepsy :—

" Amongst the out-patients to-day was one (J. H.) whom you have seen; and we suppose him cured. His history is as follows :—

" J. H., aged 19, apprentice to a carpenter; had fits five years. Had no fits in childhood; has five brothers. All the family healthy; no history of syphilis. First fit occurred after a hard day's work, having gone without dinner; it lasted half an hour, was followed by stupor of two hours' duration. A second fit happened in ten days, after a feeling of exhaustion produced by lifting timber. Afterwards, the attacks came at variable intervals, the longest being three weeks apart. They never happen in bed; usually in the after part of the day; convulsion equal; no scream; does not bite his tongue. Has no petit mal. Memory defective, especially latterly; attention good; answers questions with some anxiety and effort. Well nourished as to bulk; fair and tall; says he is physically strong. Pupils large. Has always cold feet and hands. He denies the practice of masturbation. Says he knows when a fit is about him by a sinking feeling in the belly, and his back feels weak; is always more or less flatulent, but most so before a fit; he feels a great want and a desire to eat, but cannot eat much. He shivers once or twice, then falls without further warning. A spoonful of brandy without water will often prevent the fit, then he has always loud eructation of wind. Bowels not relieved for three days together, and often not then without the aid of medicine.

" July 3rd.—Ordered extract of belladonna, gr. $\frac{1}{2}$; cod-liver oil, 3j., with four drops of the phosphorized oil of the Prussian Pharmacopœia three times daily. To have a hot pediluvium every night; and a hot shower-bath, followed immediately by a free sprinkling of cold water, every morning. To apply a strong liniment of turpentine to the epigastric region about the time of the fits.

" 10th.—The report is :—One fit of slighter character; all other symptoms better. To continue the same treatment, but increase the belladonna to one-third of a grain.

" 23rd.—No fit. Flatulence very troublesome at times; it is not so confined to the upper part of the abdomen. Better after meals. Bowels relaxed. To continue same treatment.

" On January 7th he reported having had another fit; he had been well until that time, and he attributes this attack to having left off medicine for three weeks, and to excessive eating and drinking at Christmas time. We continued the same treatment, and from that period to the present (July 10th) he has remained well. The flatulence, sinking, and all other symptoms, have entirely disappeared, and he looks in perfect health.

" This case is a fair, but not a very good, type of a very common class of epileptics. They have for an aura some disturbance of sensation, accompanied, or not, by abnormal feeling of motion in the abdomen. Patients describe these feelings variously, as turning upside down, sinking, fainting, a sense of great coldness, or a rush upwards from the epigastric region, of heat, trembling, borborygmi, shivering, or a feeling of complete collapse, and emptiness of stomach, sometimes with nausea.

" More or less of these feelings may be always present, and exaggerated only just before a fit. I call this class of cases Ganglionic Epilepsy, and I believe the symptoms complained of arise from a disturbed condition of the solar plexus and the ganglionic system of the abdomen generally. It may be from a failure of action, or from a disturbed or intermittent action of the solar plexus, and its dependent neighbouring ganglia. I believe the morbid action starting in the ganglionic system propagates itself, by way of the splanchnic nerves, to the cerebro-spinal centre, and a fit follows. But what I wish most particularly to enforce just now is that this disorder of the ganglionic system is a disease *per se*, often existing alone, and antecedent to any epileptic attack; in fact, that the epilepsy is an accident which issues from, and follows, it, and so is fundamentally different from epilepsy arising from disease in the cerebro-spinal centre, or from a distinct cause of irritation situate in any other part of the body. This ganglionic affection is as much related to hysteria, tetanus, catalepsy, and, perhaps, intermittent fever and cholera, as to epilepsy; and when I meet with such a case I think it wise to ignore the convulsive attacks for a time, if they be infrequent, or look upon them only as an index of our progress in restoring power to the great centre. I should ignore the attacks altogether were it not for the fact that each attack may, by causing congested blood-vessels, by effusions, by mechanically weakening delicate brain-structure, predispose to other attacks, or by dilating the minute and weakened vessels on the medulla oblongata, cause permanent mischief.

" On interrogating such patients, you will generally find a history of overwork, of underfeeding, of mental anxiety, of grief; amongst the female sex of exhausting discharges, as menorrhagia, of many miscarriages, of numerous children, of prolonged lactation. Indeed, many cases arising from the last cause which we meet with in general hospitals, possess every symptom that epileptics exhibit, even to the clonic convulsive action of the muscles, minus only the general convolution and loss of consciousness.

" The ganglionic system, we have shown, presides over the circulation in the brain; it enters largely into the composition of its substance, and is abundantly distributed to all the viscera in the body. But its great centre is in the abdomen, and failure of power in the central ganglion here must be followed by failure in some degree everywhere. What particular nosological disease follows must depend on predisposition, hereditary influence, or special debility of particular centres or parts of body.

" Conversely, too, disease of the cerebro-spinal centre, or of brain

proper, must influence the ganglionic system, although in a very much smaller degree, and in a more limited area; for, in the one case we have an affection of the great ganglionic plexuses in the abdomen, in the other only a local mischief, since a large amount of cerebral disease is hardly consistent with life.

"I look upon the solar and neighbouring plexuses, and the ganglia down the spine, as not only generators, but reservoirs of power. A blow on the epigastrium kills by paralysing the solar plexus; prolonged and unaccustomed exertion takes away all appetite, by exhausting the centre, as many of you must have experienced after walking all day on a first of September. Violent emotion causes fainting, acting in the same way by paralysing the great centre; fright gives rise to a very large per-cent of our epileptics; but the fright does not act so immediately, causing the fit; the ganglia are only disordered functionally at first; sometimes days or weeks of uneasiness in the epigastrium, sinking, and mental uneasiness precede the convulsion. Chorea, also, acknowledges fright as its chief cause. Neither is the effect of fear shown in at once producing chorea. A disturbance of nutrition precedes it, and the regulation of nutrition is the great function of the ganglionic system. Given, then, a case of ganglionic epilepsy, our chief and first business is to restore power to the great nervous centre. How this was accomplished in the patient J. H., is mentioned in the short history of his case."

ART. 28.—*Epileptic Aphemia with Epileptic Seizures on the Right Side.*

By Dr. HUGHLINGS JACKSON, Assistant-Physician to the National Hospital for Paralysis and Epilepsy.

(*Medical Times and Gazette*, August 13, 1864.)

"In the following case," writes Dr. Jackson, "there is no evidence of heart disease, and the patient seems very healthy. We cannot be certain that she had loss of language, but it is clear that she had at least loss of articulate language—Aphemia. In a footnote to one of his clinical lectures, Dr. Todd writes, 'There is a peculiar class of cases of epileptic hemiplegia in which the exciting cause of the epileptic fit at the same time damages or greatly injures voluntary power and speech.' In some of his cases of this kind the hemiplegia was on the left side. In the well-known case of Beglin it was on the right, and speech was lost (Beale's edition, p. 851). At the autopsy on that case there was found disease on the surface of the left side of the brain. Such disease, though a widely different thing to embolism, I believe acts to the same end in producing epileptiform seizures, by inducing, by reflex action, spasm of the branches of the artery (the middle cerebral) in which it lies. This is a mere hypothesis, but it is, at all events, a smaller one than such theories of reflex irritation as are generally received; such, for instance, as that worms or teething

produce universal convulsions ; and it gives the theory of reflex irritation, at least, an anatomical framework to say that disease in the range of a cerebral artery produces spasm of the branches of that arterial system. The view generally held supposes that an irritation may produce paralysis or spasm in parts which have no special anatomical connexion or physiological relationship. If my hypothesis be correct, the brain-substance would be temporarily ill-nourished by spasm of the vessel, just as it would permanently in the case of embolism by plugging of the arterial trunk. In the first, then, we should have unilateral convulsions, and in the second unilateral paralysis—in one hemiplegic epilepsy, in the other what, if, as is now and then the case, it were preceded by convulsions, we should call epileptic hemiplegia. Clinical observation seems to me to demonstrate beyond a doubt that convolution is closely allied to paralysis. We constantly find paralysis following convulsions, and, what is more striking, we as often find convulsions beginning in parts already paralysed.

"D. T., aged fifty-four. One day, three years ago, she was going down —- street, and suddenly lost her speech, and felt 'numb' on the right side. She then fell down, became insensible, and was, she was told, convulsed on the right side. Next day she felt well, but went to a hospital because she was alarmed. She, however, never fairly regained her speech, but had only a little difficulty in 'pronouncing her words, and did not, she says, use wrong words.

"In three or four months afterwards she had another fit. This began in the arm and leg as 'a throbbing,' and then she lost her speech.

"She has had seven or eight fits since. They always affect the right side, she always loses speech, and then becomes insensible for a few hours. After the last she could not speak for several hours.

"I need scarcely say that I ascertained that her speech was lost in all these attacks whilst she was conscious.

"The close resemblance of the temporary symptoms in the epileptiform seizure in this case to the permanent ones in other cases, in which there is undoubtedly occlusion of the left middle cerebral artery, goes, I think, to demonstrate that there was in the seizures temporary disorder (spasm of vessels?) in the circulation in that arterial system only. Further, the cases I have related, taken together, illustrate the great importance of Dr. Radcliffe's views on epilepsy generally. They show that we have—not unfrequently to treat softening of the brain, rather than epilepsy; to succour feeble nervous power, and not to combat a paroxysm; and that temporary violence is not a sign of general vigour.

"A patient may be in rude health, and yet part of his nervous system may be below par from some quasi-accidental cause, as embolism, or local injury to the side of the skull, or syphilitic deposit in the pia-mater on the surface of the hemisphere. In such cases, then, part of the nervous system only being affected, the convulsions also are partial. The disease being limited to one side, the epileptiform seizure also affects one (the opposite) side only. For instance, there being (as I have suggested is most probable in the

first case) partial occlusion of the *right* middle cerebral artery, and consequently impaired nutrition in the *part* (corpus striatum) it supplies, we have in such cases convulsions, followed by more or less hemiplegia (from 'numbness' to total paralysis) of part of the *left* side of the face, of the *left* arm, and *left* leg. It is but a difference of degree when complete hemiplegia at once follows complete occlusion of the artery. And these particular instances seem to point to a rational treatment in the same way as Dr. Radcliffe's general principles do. Without denying the great value of certain kinds of empirical treatment in epilepsy—as, for instance, the bromide of potassium, zinc, belladonna, &c.—all will agree that we ought to find our way when possible to a more rational system of therapeutics. It seems, then, reasonable to suppose that when the nutrition of a *part* of the brain is below par we should give as drugs and food such substances as are known to contain constituents of nervous tissue. Then, the blood being enriched, the part below par would get its nutrient supply more easily. Indeed, some of the patients who have these unilateral seizures are in such robust health that it seems like a practical joke to give general tonics like iron, quinine, &c. But in a utilitarian profession like ours we are obliged to look to results. Dr. Radcliffe, then, finds that hypophosphites and cod-liver oil are most satisfactory agents in the treatment of epilepsy, so that they may be given on the broad principle that they nourish nervous tissue, or on the narrower theory that they do good in the particular disease, epilepsy."

ART. 29.—*On Epileptiform Neuralgia.*

By M. TROUSSEAU.

(*Journ. de Méd. et Chir. Prat.*, January, 1864.)

In some clinical remarks recently delivered at the Hôtel Dieu, Paris, M. Troussseau dwelt on a condition which he acknowledges to have for many years, like others, confounded with facial neuralgia, or neuralgia of the fifth pair, but which, by its analogy with painful *aura epileptica*, now appears to him to merit a distinct place amongst the varieties of neurosis, under the denomination of *epileptiform neuralgia*.

This distressing and spasmodic affection well deserves the denomination of *tic douloureux*, and differs from genuine neuralgia of the face by the suddenness of its appearance, the excruciating intensity of the pain, the entire cessation of all suffering after a duration pretty constantly the same, and the impossibility of relieving the paroxysms, which recur at variable intervals of one or ten minutes, one or two hours, &c. Some two years since, a patient in M. Troussseau's wards experienced thirty attacks in the hour; he could neither eat, drink, nor sleep, and this torture persisted sometimes for two successive months without interruption. M. Troussseau was once consulted at his house by a prefect who had scarcely taken a seat when he suddenly sprang up, and applying his

hand to the right side of his face, which was in a state of spasmodic convolution, walked up and down the room stamping and swearing like a madman. This strange scene lasted about a minute, when the pain subsided, and the patient apologized for his violence and agitation. M. Troussseau assured him he fully understood the nature of his sufferings, which he might mitigate, but could not promise to remove altogether. This peculiar form of neuralgia, which affects the trifacial nerve only, never yields in a complete and permanent manner.

Thus without proscribing the section of the nerve, which M. Troussseau might even now resort to in a case of the kind, he greatly doubts whether the procedure can be expected to produce lasting results. Blisters dressed with morphia, hypodermic injections, &c., are utterly unavailing. One remedy, and one only, proves an efficient palliative, viz., large doses of opium.

M. Troussseau brought forward in support of this statement the case of an aged lady from the city of Antwerp, who was a martyr to this kind of neuralgia, which was allayed with enormous doses of opium. The duration of each paroxysm varied from a few seconds to three minutes, and the pain attacked in turns the three main branches of the nerve, rapidly extending to every filament, and inducing convulsive action of the muscles of the jaw. Twenty of these fits occurred within an hour. An interval of a fortnight or a month sometimes elapsed without recurrence of the disorder, but it invariably returned with the same violence. In this instance, M. Troussseau prescribed each day at first three or four grains of muriate of morphia, which he gradually increased to one drachm. Great improvement was effected, but the treatment being expensive, the professor obtained for the patient the drug at cost price, when she took as much as two drachms a day with much relief, and, singular to say, without any disturbance of the digestive functions. The tolerance consequent on habit is sometimes incredible; M. Troussseau stated that in one instance he had seen a brushmaker drink several full glasses of laudanum. It is, however, prudent to exhibit the medicine in very gradually increasing doses, and to watch carefully its effects on the system.

ART. 30.—*On the Use of Tea as a Remedy in Coma.*

By Dr. SEWELL, Physician to the Hôtel Dieu, Quebec.

(*Lancet*, July 16, 1864.)

The late Dr. Graves recommended the use of tea in the coma of fever, and this recommendation led Dr. Sewell to think that the same practice might be useful in cases of an analogous kind. Four cases are given, of which the following is one:—

CASE.—Mrs. A. B—, aged 30, has been subject for some years to what she calls “spasms of the heart,” for which she some time ago visited Europe and was treated at different times by Drs. Simpson of Edinburgh,

Stokes of Dublin, Troussseau of Paris, and other eminent men in London, Vienna, and Glasgow, but without any effect. Latterly she had been using Battley's sedative solution with more benefit, but as she had no attack for some months she had discontinued the use of this remedy for about three months. A short time since she was threatened with one of her usual paroxysms, and, dreading it very much, she had recourse at once to Battley's sedative solution in two-drachm doses; these doses she continued at intervals till she had taken two ounces and a half in about eight hours. Shortly after the last dose she was seized with a slight convulsion, and almost immediately became comatose. I saw her at two o'clock A.M., two hours after the convolution, and found her in a state of profound coma; pupils contracted; respirations two in the minute, and performed with a great effort; pulse very rapid, small, and extremely irregular; face deathly pale, ghastly, cold, and covered with a clammy sweat; extremities also cold. It was evident that she was under the narcotic influence of the enormous dose of opium which she had swallowed, and that death was imminent. As three hours had elapsed since she had taken the last dose, I conceived it useless to use the stomach-pump; moreover, in the then state of her respiration, I believe the use of that instrument would have been at the risk of her life. As she could not swallow, an emetic was equally out of the question. I therefore applied extensive sinapisms to the legs and chest, used the cold douche, and applied ice to the head. Having by this time been joined by my friend, Dr. Jackson, I suggested the propriety (while waiting for a galvanic battery) of administering an injection of a pint of the strongest possible infusion of green tea per anum, which was done at a quarter-past three A.M. In half an hour there was a visible improvement in the breathing, which was now six in the minute, accompanied by a slight return of colour to the face, and a corresponding improvement in the temperature of the cheeks. The coma continued much the same, but, encouraged by the improvement in the other symptoms, the injection of tea, to which some brandy was added, was repeated at four o'clock. During the next hour we had the satisfaction of observing a gradual return of the respiration to its normal condition, with an improved state of the pupils, and a corresponding change in the general temperature of the body. She continued to progress favourably, and between five and six o'clock (or about two hours and a half after the first injection), though she could see nothing, she recognised those about her by their voices, and soon after we were enabled to pronounce her out of danger.

ART. 31.—*On the Curability of Hydrophobia.*

By M. DECROIX.

(*Commission sur-la Rage; and Glasgow Medical Journal, July, 1864.*)

During the year 1850, a distinguished veterinary surgeon in the French army, M. Decroix, while he was in garrison at Algiers, made some experiments, with a view to prove, first, that hydrophobia exists in the above town (in contradiction with the general opinion), and secondly that this terrific disease is susceptible of being cured.

In a first experiment he inoculated a dog with the foam of a mad one, the former, after having shown during seven days very visible symptoms of hydrophobia, got spontaneously cured on the twenty-fifth day after the inoculation.

In a second experiment, a dog was inoculated with the saliva of a man labouring under hydrophobia. The inoculation perfectly succeeded, which proves that hydrophobia can be transmitted from man to dog. The subject of this experiment became mad at the end of sixteen days, showed signs of madness for about a week, and became entirely cured about one month after the inoculation.

M. Decroix, seeing this favourable result, doubted for a time the accuracy of the diagnosis. But his scruples were dissipated when he learnt that, according to the testimony of the Professors Lecocq, Rey, Tisserand, and Tabourin, several cases of cure of hydrophobia by the sole efforts of Nature had been observed at the Veterinary School of Lyons.

The curability of hydrophobia, is therefore, in the belief of M. Decroix, quite a recognised fact. His experiments prove, he says, that hydrophobia can be cured spontaneously. He does not much believe in specifics; but for all that, he thinks that practitioners should not cross their arms and remain inactive in such a disease.

First of all, you must reassure the people bitten by suspected dogs, and use the most energetic and diversified means to keep away their thoughts. M. Decroix is convinced by himself of the influence that these preoccupations can exercise upon even the most unimpressionable natures.

This veterinary surgeon narrates that on the 25th November last, he himself swallowed, having nothing in his stomach, a bit of raw meat, coming from a dog dead only a few hours previously from hydrophobia; and that on the 29th of the same month he ate two pieces of roast meat also coming from a mad dog. M. Decroix, convinced up to that time that the flesh of mad animals could be eaten with impunity, took no notice of his experiments, when a few days afterwards he read, in an article of Professor Lafosse of Toulouse, that several authors had pointed out cases of transmission of hydrophobia by using the meat of mad dogs or herbivora. M. Decroix felt then his security lessening, and nearly immediately was attacked with a peculiar sensation in his throat and a notable alteration in his voice. These phenomena disappeared under the influence of diversion or arduous occupations; but they reappeared sometimes in moments of inaction, or when M. Decroix thought of hydrophobia or of the experiments of the 25th and 29th November. Hence this example may be added to those already quoted, to show the influence of imagination and of preoccupation in the development of hydrophobia in man.

To leave the patients in the greatest calm, not to contradict them, satisfy their wishes, procure diversions for them, and keep up their spirits, shun the use of means which experience has shown to be useless or injurious,—such are, according to M. Decroix, the principal things to be done in the treatment of hydrophobia.

ART. 32.—*A Singular Case of Hydrophobia (?) Seventeen Years after the Bite of a Dog.*

By Dr. S. G. CHUCKERBUTTY.

(*British Medical Journal*, September 24, 1864.)

CASE.—James C.—, aged twenty-four, an indigo-planter's assistant in the Mofussil, was brought down to Calcutta in a most helpless and deplorable state. The day after his arrival, I was called in to see him, when I found him in the following state.

He was struggling in a fit of convulsions, into which he had been thrown by the announcement of my presence. The mind was perfectly clear, and the speech rational, though interrupted by muscular spasms. The eyes were starting and rolling restlessly. There was a strong expression of alarm in the countenance, and violent working of the muscles about the neck ; the patient crying out, gaspingly, "I am dying! I am dying!" repeatedly. The body was inclined ; the legs resting on the bed, and the head and shoulders raised by two persons. The whole frame was convulsed ; and he snapped at any one who came within reach of his teeth. His mother, father, and sisters, were thus severely bitten in several places. The fit lasted about two minutes.

When tranquillized, he spoke well and intelligently ; but on trying to give him some drink, the fluid was violently rejected, and there came on dreadful spasms of the throat, with a sense of choking, which made him jump up into the sitting posture and pitifully implore for help. His parents stated, that he had had many similar fits all through the night since his arrival, though his bowels had been freely moved by some purgative which had been obtained from a druggist.

On inquiry, I learnt that he had been ill in this way for three days in the factory, when another assistant, discovering the state in which he lay, immediately brought him down to Calcutta, and turned him over to his family. This was on January 24th, 1864 ; and my first visit was paid on the 25th. He was bitten by a dog only once in his life, and that was seventeen years ago, when quite a boy, by a pet animal. He had not been bitten by any other animal. His health generally had been good, except that he had long suffered from spermatorrhœa, for which he had been under my own treatment at different times. Latterly, he had worked very hard, riding about the whole day, and exposing himself a great deal to the sun. His meals were irregular, and he never took stimulants of any kind—his drink being pure water. A sense of fatigue seemed to creep over him gradually, till at last he was suddenly seized with convulsions and general tremors, incapacitating him for work and even for rising from his bed. He had indulged much in venery long ago ; and there was a small chancre on his penis at the time.

With regard to his present state, he was very anxious to learn what was the matter with him. He told me that he felt very curious in his head, and that, though he could not drink water comfortably, the application of it to the vertex gave him great relief. His appetite was good ; but he could not sleep for fear of choking.

On further examination, I found his urine abundant and clear ; skin of natural warmth ; face slightly flushed ; pulse weak but frequent ; abdomen soft : loins free from pain ; spleen and liver healthy ; heart's action and respiration good (though hurried during the fits).

In addition to certain purgatives, already mentioned, he had had some port wine and sago.

I continued the port wine, and ordered him as much solid food as he could eat. I also prescribed for him ammonia, sulphuric ether, compound tincture of lavender, and camphor mixture, every half hour; a gentian and opium pill at bed-time; and anodyne friction all over the body; leaving the chancrre to be dressed with black wash, as he had been doing.

On the 26th, matters were much in the same state; he had passed a very bad night; several times his family thought he was expiring. The mixture had been given him regularly; and latterly he was able to swallow some of it. Since the morning, too, the snappishness had disappeared, and he had slept a little. He had taken food pretty well, and the bowels were freely moved; the stools and urine were natural. The same treatment was ordered to be still continued, with the exception that beer was substituted for port wine.

On the 27th, the restless rolling of the eyes and muscular agitation were somewhat less, though the patient was still afraid to turn in his bed for fear of convulsions. The power of swallowing had increased, so that he took his medicines regularly. He drank, also, broth, and water; ate some calves' feet jelly; and was inquisitive as to what I thought of him. The bowels were constipated. He took to-day a dose of castor-oil, which operated well; and continued the same medicines as before.

On the 28th, the rolling of the eyes had ceased, the jerking of the head was less, and he had had no convulsive fit. He could sleep now without opium; and wished to know if he could bathe, as the application of a wet towel to the head gave him great relief. He also complained of weakness, and wanted something to give him strength. The opium now was entirely omitted; and sulphate of iron and gentian pills were given instead. The mixture was continued every hour. He took now some mutton-chops with relish. His clothes, also, were changed.

On the 30th, he was stronger and better, though still unable to rise, passing his excrements in bed. So long as he lay perfectly still, there was no agitation of any kind; but the moment he attempted to move, the whole body was thrown into a state of tremor. The appetite and sleep were remarkably good; and he ate well. He was told to take three pills a day, and the interval between the doses of the mixture was increased.

On Feb. 3rd, he could sit up; and was allowed to take a warm bath, which stopped the convulsions for the day till bed-time.

After this, his progress was steadily satisfactory, under the influence of generous diet, beer, sulphate of iron and gentian pills, warm baths, and gentle airing.

On March 4th, he paid me a visit at my house, and stated that he felt as strong as ever, though still unable to walk by himself.

In two weeks more, he was able to return to his work.

*ART. 33.—Long Standing Trifacial Neuralgia cured by
Excision and Cauterization of the Nerves.*

By M. NÉLATON.

(*Journ. de Méd. et Chir. Prat.*, Juin, 1864.)

This case is that of a patient who was recently in M. Nélaton's wards at the Hospital of the School of Medicine at Paris. It is

related in the Hospital Report by the writer of this Report, not by M. Nélaton.

CASE.—A man of about forty came up to Paris from the Département des deux Sèvres, to consult M. Nélaton as to the possibility of obtaining relief from excruciating pains in the right side of the face.

Ten years ago, the patient suddenly experienced, without any known cause, intense pain in the right cheek; he described his sufferings as agonizing; the pain, at first, lasted but a few minutes, and returned at irregular intervals, but, after a time, it became more frequent, and at last incessant. In the incipient stage of the disease, the pain exclusively occupied the mental foramen, but soon reached the infra-orbital nerve and the cheek; thence it extended to the nose, eyelid, and ear; the brow was slightly affected, but the tongue and other parts, which receive filaments from the fifth pair, enjoyed entire immunity—a circumstance which appeared to M. Nélaton to show that the neuralgia was situated in some of the divisions of the nerve, and not in the main branch.

When the patient was admitted into the wards, the pain was incessant and occasionally became excruciating, either spontaneously, or from the mere contact of a spoon, a glass, or a morsel of food. It is needless to say that many remedies had previously been resorted to. Sulphate of quinine, atropia, morphia, had in vain been exhibited; and the actual cautery, and even the division of the nerve, had been unsuccessful. Alcohol only, in large doses, had given some temporary relief; but this was a mere palliative, and the transient alleviation was always followed by increased suffering.

Under these circumstances, M. Nélaton felt some embarrassment. In a score of similar cases he had performed incision and cauterization of the nerve without advantage, and yet these were the only available resources. In three cases, it is true, he had derived permanent benefit from these procedures, and almost always noted after the operation an improvement, of short duration doubtless, but which the sufferers had considered an inestimable boon. He conceived himself, therefore, fully justified in giving the patient the benefit of a procedure uncertain, it is true, in its ultimate result, but which it would have been inhuman not to attempt, as its possible success might restore to health one to whom, in his present condition, death itself appeared a relief. M. Nélaton endeavoured to effect his purpose as follows:—Reflecting on the insufficiency of mere section of the nerve, which, in this instance, had already failed, and the necessity of adopting a plan calculated utterly to prevent, or, at least, to render highly improbable, the union of the divided nervous filaments, M. Nélaton resolved on the excision of a part of the nerve and on cauterization of the extremity of the upper segment.

In the present case, the pain occupied three distinct regions, and a portion of three separate nerves must, therefore, be removed—viz., the mental, buccal, and infra-orbital. After emerging from the foramen, the mental nerve vertically ascends towards the interval which separates the first from the second incisor, and may be exposed by a horizontal incision of the mucous membrane and periosteum. On raising the latter, the nervous filaments are readily discerned. About four lines of the nerve should be excised, and a piece of Canquoin's caustic (chloride of zinc paste) be inserted into the foramen. The data are less distinctly marked for the surgeon's guidance for the infra-orbital nerve, and the operation is, therefore, somewhat more difficult. The nerve here escapes from the foramen at the junction of the inner third of the inferior margin of the orbit with the two outer thirds, and at about two lines from the edge of the bone. A curved incision, parallel to that edge,

must therefore be performed a little below the foramen, in order that the dissection may be carried on at a proper distance from the eyelid. The buccal branch runs across the internal aspect of the coronoid process, nearly at the middle of the bone. Hence an incision of the mucous membrane in this region readily exposes the nerve, which can then be easily divided.

M. Nélaton, guided by these precise indications, unhesitatingly performed the three operations with the greatest expertness. An equal portion of each nerve was removed, and a piece of caustic was inserted into each foramen. No chloroform was exhibited; and although the proceedings were extremely painful, the patient bore his sufferings with unflinching courage. All sensation immediately ceased in the parts to which the filaments of the divided nerves were distributed, and the patient slept that night for the first time for many months. The following day he was enabled to eat and drink without pain. After a few days the neuralgia threatened to return, but did not persist. The operation was performed on the 27th of January, on the 10th of February the man was discharged from hospital, and on the 10th of March he wrote to express his heartfelt gratitude for the complete and lasting relief which had been afforded to him.

ART. 34.—*On the Use of Turpentine Capsules in Nervous Headache.*

By M. TEISSIER.

(*Gaz. Méd. de Lyon*; and *Journ. de Méd. et Chir. Prat.*, Mai, 1864.)

The headache to which reference is here made differs in several essential points from genuine neuralgia. It is more rapid and continuous; it is marked by less distinct and less frequent intermissions. It may be of a dull or occasionally of a shooting or throbbing character; it may occupy one spot only, or be dispersed over the scalp. Serious complications, such as giddiness or faintness, impossibility to exercise the mental faculties, *tedium vita*, and even disturbance of common sensation and numbness of the extremities may be present. This form of nervous headache is peculiar to delicate, anaemic, and more especially, hysterical females. It often is accompanied by dysmenorrhœa, amenorrhœa, or profuse menstruation, but is also sometimes observed in persons whose functions are regular, and who present all the outward signs of vigorous health. It may be idiopathic, sympathetic, or symptomatic, and is not unfrequently connected with herpetic or rheumatic tendencies.

In addition to the remedies calculated to improve the tone of the system, M. Teissier strongly recommends essence of turpentine.

It should be administered in the shape of capsules.

M. Troussseau advises the exhibition of four or six pearls or capsules of turpentine twice daily. Graves was in the habit of prescribing half a drachm or one drachm of the drug in the course of the day. These appear to M. Teissier unnecessarily large doses. The therapeutic effects of the remedy are evolved by the administration of two or three capsules in the day, at meal-time. Each pearl or capsule contains about eight drops of the essential oil; and

with sixteen or twenty-four drops of so active a remedial agent, important results may be effected. In the more obstinate cases only, M. Teissier ventures on larger doses.

ART. 35.—*Case of Progressive Muscular Atrophy.*

By DR. EDGAR BARKER.

(*Proceedings of the Royal Medico-Chirurgical Society*, June 14, 1865.)

In the discussion following the reading of this case, Dr. Meryon and Dr. Gull made some remarks which deserve to be preserved. Dr. Meryon said that in a similar case to that related, the spinal cord and the nerves connected with it had been examined, but not the slightest disease could be detected. Cruveilhier had recorded three cases of the disease, in two of which the spinal cord was not affected, though it was in the third case. In this patient there was the same tremulous action of the muscles. The origin of the disease had been attributed to exposure to night air. Since his (Dr. Meryon's) case had been related, two brothers of the patient had died, the last one about three years since. Dr. Meryon then referred to several cases of the affection, in none of which had he seen the tremor of the muscles, except after the use of electricity. He attributed the disease simply to a breaking down of the muscular structures of the part, and thought it unconnected with nervous changes. Medicines in these cases had exerted little or no influence. In one case, however, he had given liquor arsenicalis for six months, and the disease had not progressed.

Dr. Gull said that the case referred to by Dr. Meryon and the one under consideration were not at all similar. "Dr. Meryon's was a class of cases occurring amongst young people, and the muscular degeneration was somewhat analogous to the rickets in bones. The case of Dr. Barker was of another kind; and considering that no microscopical examination had been made of the seventh and lingual nerves, it was valueless in respect to the cause of the disease. He entered his protest against the assumption that it was primarily dependent on muscular degeneration. How did it commence? Now, looking at the fact that one of the olivary bodies was flattened and the anterior pyramid altered in form—the very eye, as it were, of the nervous centres,—and taking into account the symptoms, it would appear that the disease had its origin in the nervous system. He had shown in some lectures at the College of Physicians that paralysis might begin in any of the structures of the body."

CASE.—The subject was a gentleman, aged fifty-one, who had enjoyed excellent health till May, 1859, when a slight difficulty of speech, accompanied by general failure of health and strength, induced him to seek medical advice. These symptoms, without any apparent cause, with the addition of impairment of deglutition, continued to increase; and in the following September, after some months' residence at the sea-side, the tongue had assumed the following remarkable appearance: small and shrunken, it lay low in the floor of the mouth, and over its whole surface was noticed an unceasing tremulation of the fibrils of its muscular struc-

ture ; it had lost its bright healthy hue, and was of a pale yellow colour. His face had also lost its ordinary expression ; the cheeks and lips were flaccid, and hung down. Saliva frequently dribbled from the mouth. No symptom whatever of irritation of brain or spinal cord was ever present, but the muscular tissue in different parts continued to waste and degenerate with unrelenting pertinacity. Gradually articulation became unintelligible, and deglutition impossible. The fibrillary tremors so noticeable during the wasting of the muscles ceased with their destruction. From the tongue to the muscles of deglutition, thence to those of the upper and from these to the lower extremities, the disease extended. At length the intercostals were affected ; and the breathing consequently at times became much laboured, as each morning brought increasing difficulty in the necessary expulsion of mucus collected in the bronchial tubes during the previous night. Great exhaustion followed these attacks, and on the morning of the 15th of October, 1861, he gradually sank. Various remedies had been for many weeks together tried, but none seemed in any way to arrest the steady onward progress of the disease. Cod-liver oil, quinine, iron in various forms, zinc, strychnia, and the constant use of galvanism, were the principal agents employed. The post-mortem examination of the tongue went to prove that in its entire extent it had been converted into a soft, pale yellow mass of fatty tissue. The papillæ were shrunken, and most of its muscular fibres were replaced by oil-globules, amidst which granular and fat-laden fibres were here and there scattered ; and of the muscles attached to the tongue, only the genio-hyo-glossi and stylo-hyo-glossi retained any manifest traces of their form and structure. The nerves of the tongue, so far as traceable, were natural ; no apparent softening or atrophy of them could be detected ; muscular fibre in the arches of the palate and in the uvula were chiefly natural, save here and there. The same granular appearance was noticed in the pectoralis major and in a portion of the left ventricle of the heart, and in the left side of the diaphragm. In all, the muscular fibre was in great part natural, though each specimen in an equal degree contained stray fibres, which were losing the clearness of their transverse markings, and becoming granular with fatty deposit. The examination, worked out with the greatest care and by accurate observers, failed to bring satisfactory evidence of any change in the nervous tissue supplying the affected muscles, either in their centre or peripheric extremities ; but, on the contrary, the examination tended to strengthen the present prevailing opinion that the disease is essentially in the muscular tissue itself, and must yet be looked upon as akin to that condition frequently met with in the left ventricle of the heart, and known as fatty degeneration.

**ART. 36.—*On the Condition of the Sympathetic Nerve in
Ataxie Locomotrice Progressive.***

By M. DUCHENNE (de Boulogne).

(*Gaz. Hebdo. Q. Sér.*, 1, 8, 10, 1864; *Schmidt's Jahrb.*, No. 5, 1864.)

Duchenne has observed, in some cases of ataxy, strong contraction of the pupils in the ordinary state, which, however, during the paroxysms of pain gave place to dilatation ; accompanied by marked injection of the conjunctiva with increased temperature, but without catarrhal secretion : further, the dilatation of the pupil was not producible by atropine. Occasionally the dilatation took place

without the other symptoms, and at times contraction alone occurred. Also, these symptoms were sometimes only developed on one side, sometimes on both. Duchenne ascribes these phenomena to an affection of the sympathetic in the neck, and connects the latter again with the causes of the ataxy. Accurate dissection alone can indicate how far these causes are found in material changes: so far, examination of the cord has elicited nothing. The lesions in the eye are probably analogous to the alterations in the activity of the bladder, the rectum, and the genital organs, which it is equally impossible to trace to grey degeneration of the posterior columns of the cord. Besides, the latter affection is not constantly present (the degeneration being sometimes in the anterior columns), and it is sometimes observed in other affections, e. g., progressive muscular atrophy. Evidently the pathological genesis of ataxy is not yet properly understood, nor its symptoms reconciled with the dicta of physiology.

ART. 37.—*Differential Diagnosis of Diseases of the Cerebellum and Progressive Locomotor Ataxy.*

By Dr. DUCHENNE (de Boulogne).

(*Gaz. Hebd. de Méd. et de Chir.*, Juillet 15 et 29, 1864.)

Diseases of the cerebellum and progressive locomotor ataxy are easily confounded together from the apparent identity of the functional disturbance of locomotion in both cases. The author himself, before the pathological anatomy of locomotor ataxy was sufficiently known, had attributed the disorders it produces in the maintenance of equilibrium, and in the harmony of antagonistic muscles, to some probable lesion of the cerebellum. This conjecture was based on physiological experiments of Flourens and Bouilland, experiments which had been apparently confirmed by observations made in man.

When, however, it had been clearly shown by the microscope that the cerebellum is perfectly healthy in locomotor ataxy, whilst pathological lesions are met with in other portions of the nervous system; the author foresaw that, by carefully analysing them, the functional disturbances of locomotion characteristic of diseases of the cerebellum might be distinguished from those due to locomotor ataxy. After a careful study of the symptoms observed in diseases of the cerebellum, he came to the conclusion that these diseases produce a kind of intoxication of the power of motion, and not an incoordination of movements. This opinion he communicated to the Medical Society of the Seine, and intended to substantiate it by the post-mortem examinations of several cases under his care that were likely to be soon terminated by death. A critique of his views by Dr. Voisin, however, prompted him to prove at once the truth of his assertion. Dr. Voisin, in fact, lays down the following propositions:—

1st. The disturbances of locomotion due to diseases of the cere-

bellum and to locomotor ataxy are perfectly identical, and cannot be diagnosed from one another.

2nd. The cause of the disorders of motility in locomotor ataxy must be some pathological condition of the cerebellum.

This opinion, Dr. Duchenne observes, is exactly the same as the one he held himself when he first published the clinical history of locomotor ataxy; but he has since discovered his mistake, and now honourably confesses it. The disturbances of locomotion occurring in diseases of the cerebellum are due, he says, not to a want of power in coordinating movements, as taught by physiologists, but to a kind of intoxication like alcoholic intoxication. In locomotor ataxy, on the contrary, there is no vertigo, no sensations like those felt in a state of drunkenness. It is true that the difference in the symptoms, taken as a whole, must sooner or later point to whether the case is one of cerebellar disease or of locomotor ataxy; but cases might arise in which the disturbances of locomotion were the principal element of diagnosis. For instance, the characteristic pains of locomotor ataxy may be absent in the first period of the disease (a rare occurrence, according to Dr. Duchenne only in proportion of 1 to 100, and even then they come on later); or the patient may conceal them, as he had known some to do, and in such case the diagnosis may have to be made immediately. Weakness of sight, paralysis of the motor nerves of the eyeball, diplopia, and impotence, may severally or individually exist in both locomotor ataxy and cerebellar disease; so that the signs which distinguish titubation with vertigo from titubation from a loss of coordinating power, can alone help in solving the problem.

The author then proceeds to contrast the movements of a man in a state of drunkenness with those of a patient suffering from locomotor ataxy. When a drunken individual, he says, is in the standing posture, he oscillates in all directions from side to side, from before backwards, or from behind forwards. The extent of the oscillations is directly proportionate to the degree of intoxication, and the drunken man quietly gives way to them. In locomotor ataxy, on the contrary, the oscillations are sudden, of short duration, come on, and go off rapidly, and do not resemble swinging. The patient looks like a tight-rope dancer who tries to stand on the rope without a pole to balance himself.

The muscular contractions, which during locomotion cause sudden oscillations of the trunk, resemble spasms. On exposing the lower limbs, certain muscles are then seen to contract powerfully and suddenly for a very short period. These contractions however are functional, not spasmoidic; they are always intended to maintain the equilibrium of the body, for if the patient oscillates in a certain direction, those muscles only are rapidly thrown into contraction which incline the body to the opposite side. They are instinctive also as well as voluntary; for it sometimes happens, especially at the outset when the incoordination is slight, that the patient unconsciously oscillates a little, and even then slight and rapid contractions may be observed of the muscles which move the feet and the toes. The wife of a patient suffering from ataxy informed the author

that before her husband had noticed he had any difficulty in maintaining his equilibrium, she had remarked that whenever he stood with his boots off, his feet did not lie flat on the carpet, but were continually agitated, whilst his toes were in turns constantly raised and lowered ; at the same time she saw his body oscillate slightly.

To these instinctive contractions are soon superadded voluntary ones, for as soon as the patient has become conscious of his losing his equilibrium, he is constantly thinking of it and making, in order to preserve it, efforts which soon exhaust him ; hence he cannot long remain standing without feeling fatigued. When the loss of co-ordination attains its maximum, just as when alcoholic intoxication is at the highest point, standing becomes impossible ; in the former case, if the patient be supported from under the arm-pits and be then asked to try and stand, his legs give way, whilst he still preserves the power of executing, with force, partial movements with his lower limbs when in the lying posture.

The gait of a drunken man differs from that of ataxy ; he cannot walk straight, and describes zigzags, goes alternately from right to left, and *vice versa* ; he inclines his body to the side to which his course deviates, and this inclination is sometimes so great that he falls, borne down as it were by the weight of his body ; he does not strike the ground with his heel, but drags his legs instead. How different is the mode of progression in ataxy. The patient walks in a straight line although he vacillates ; he describes no curves, the oscillations of his body are worse than when he merely stands ; he feels anxious, and keeps his eyes fixed on his legs. When the loss of coordination becomes considerable, the leg is thrown forwards beyond the ordinary step, and the heel comes down with force.

One of the first effects of alcoholic intoxication is to produce giddiness, vertigo ; everything seems to go round and round, and on this account the individual keeps tripping up. In locomotor ataxy there is no vertigo, the difficulty of maintaining his equilibrium is by the patient felt in his legs, not in his head. It should be remembered, however, that diplopia, sequential to strabismus, sometimes produces giddiness during progression, and makes the individual deviate from the straight line ; but this cause of error is easily avoided by causing the patient to close the eye which squints.

The author next contrasts the disturbances of locomotion observed in cases of diseased cerebellum with those peculiar to ataxy. He mentions two cases in which the diagnosis made during the patient's life was verified after death.

The 1st case was that of a watch-maker, aged twenty-eight, admitted into the Lariboisière Hospital, und r M. Cusco, in May, 1863. He had strabismus, and he kept tripping up when walking. When he was made to stand, his body oscillated in all directions with a kind of swinging movement, uninterrupted by sudden contractions of the muscles tending to maintain its equilibrium. If he walked, these oscillations increased in extent ; he described arcs of a circle, zigzags, like a drunken man. He besides complained of vertigo, and sometimes of nausea. On questioning him, it was ascertained that towards the

end of the year 1862, he became subject to vomiting, recurring several times every day, and usually after his meals. He began also to squint. At the time of his admission there was dulness of aspect, slowness of comprehension. Sight misty; general sensibility obtuse; slight diminution of power, more marked on the left side; impotence, and nausea on stooping. As he had had secondary syphilis, a syphilitic tumour seated in the cerebellum, and extending to the cerebrum, was diagnosed. Death was preceded by acute delirium, with alternating periods of excitement and depression.

Dissection disclosed no lesion of the spinal cord, or of the thoracic and abdominal viscera; but on removing the calvarium, there was found marked injection of the pia mater covering the convex surface of the brain. On attempting to remove the meninges at the back, there came away with the dura mater a portion of the cerebellum corresponding to the left lobe, and covering a space measuring about two centimètres in its circumference. There was intimate adhesion between the meninges and the nervous substance. The only fact noticed besides was rather marked injection of the whole encephalon—more apparent in the cerebellum.

The 2nd case was that of a tin-smith, aged twenty-one, admitted into the Hôtel Dieu, under Dr. Vigla's care. The principal features of this case were: deafness of the left side, vomiting, frontal cephalgia, vertigo, loss of sight, peculiar gait like that of a drunken man; diminution of sensation and power of motion in the left leg. Dissection showed the existence of fibro-plastic tumours, one of which, of about half the size of a closed fist, was situated above the corpus callosum, and compressed both hemispheres of the brain slightly, chiefly the right. Another tumour the size of a walnut lay on each middle peduncle of the cerebellum. Numerous other tumours of the same nature were also found in the spinal cord, in the roots and trunks of the spinal nerves.

Dr. Duchenne gives the details also of two other cases in which nausea, vomiting, and vertiginous titubation, exactly as in the preceding cases, pointed to an affection of the cerebellum, but no post-mortem was made. One of the patients was cured after a two-months' stay in the hospital, and the other left at the end of three weeks in the same state as when he was admitted. In both cases the disease set in suddenly without any assignable cause, with oscillations of the body, vomiting, impairment of sight, and retention of sensibility and power of motion. The patient who got perfectly well, complained of diplopia after looking at an object for some little time fixedly, and if he persisted in looking at it, nausea and even vomiting supervened. If he closed his right eye and looked at an object with his left one, he, after a few seconds, saw the object double.

In addition to the above, Dr. Duchenne has been enabled to confirm his views in four other cases of disease of the cerebellum, which he has seen at variable intervals in different Paris hospitals. From the facts observed by him, he therefore draws the following conclusions:—

1st. The disorders of locomotion in diseases of the cerebellum resemble those due to alcoholic intoxication.

2nd. Like the latter, they are solely owing to vertigo, or, in other words, they are characterized by vertiginous titubation.

3rd. This vertiginous titubation can be easily distinguished from the titubation resulting from loss of coordinating power in cases of progressive locomotor ataxy.

ART. 38.—*On the Physiological Meaning of Infra-Mammary Pain.*

By Dr. S. MARTYN, Physician to the General Hospital, Bristol.

(*British Medical Journal*, September 10, 1864.)

Dr. Martyn's definition of infra-mammary pain is this:—

"Pain, very common, either continuous or intermitting irregularly, and brought on by movement. *Sex*, more frequent in women ($20 : 5$). *Locality*, superficial, and confined to spots in the sixth, seventh, and eighth intercostal spaces, those oftenest affected being infra-mammary, the next in frequency near the spinal or sternal end of those intercostal spaces. *Side affected*, mostly the left ($13 : 7$, Valleix), sometimes both. *Temperament*, the nervous. *Duration*, indeterminate, according to Hasse, often through other diseases till death.

"It is not a rheumatic pleurodynia; for the pain is not so violent nor so diffuse, nor is there a febrile state. It is not dry pleurisy nor angina; for the breathing is free, and the lung-sounds are normal. It is not meningitis of the cord, nor softening, nor caries of vertebrae; for from all these a diagnosis may readily be made. It is a true intercostal neuralgia.

"Now, in the cases under my own observation," says Dr. Martyn, "I have found two conditions which seem never to be absent: the one, a state of general muscular weakness; the other, increased rapidity of the heart's action, except when the lowest amount of propulsive force is required, as in perfect repose of body and mind, when the pulse is often abnormally slow. Of these two points, the disordered action of the heart was the one which attracted my attention; and though I am aware that Professor Valleix, in his essay on dorso-intercostal neuralgia, does not allow any peculiarity of the pulse, I have satisfied myself by all subsequent observation of my correctness on this point. In cases of intercostal neuralgia, it will be found that there is palpitation on any exertion, and that in most cases the pain and the palpitation have a distinct connexion. The question was this: Why is pain almost invariably confined to the sixth, seventh, and eighth intercostal spaces of the left side? The answer seemed to be—a relation to the heart.

"Reflected or radiated pain is now recognised as extremely common. In cases of sensory nerves entering a great centre at the same spot, if the peripheral end of one be irritated, we feel pain in, or refer it also to, the periphery of the other. The irritation brought by the

phrenic from the diaphragmatic pleura is referred to the end of the super-acromial branches of the cervical plexus; uterine irritation is referred to the cutaneous region of the sacrum; and so on in other instances. The question thus arose, Are there any nerves supplied to the heart and to the sixth, seventh, and eighth intercostal spaces from the same central region? and then there appeared a most important anatomical relation to be taken into account. The aortic arch impinges on the left side of the third dorsal vertebra; and opposite the fourth, fifth, and sixth, it receives contributions to its plexus from the corresponding ganglia of the sympathetic, while its plexus again contributes to the heart. These sympathetic ganglia have, however, just received branches from the intercostal nerves themselves; and so it is that the heart and the intercostal spaces (four, five, six) are supplied by branches of the same nerves. Moreover, this is on the left side and above only, for on the other side, most of these branches go to the oesophagus, while below, those from the sixth ganglion chiefly go to form the splanchnic nerves. Now the fourth, fifth, and sixth intercostal nerves are those which give off large lateral cutaneous branches, descending over two ribs before they terminate in the skin over the sixth, seventh, and eighth intercostal spaces, or the site of the infra-mammary pain. Smaller twigs of the same nerves supply the ends of the same intercostal spaces, where the pain may also be perceived.

"I had made up my mind as to this nervous circuit, but felt the great difficulty in attaching any particular importance to the first few dorsal spinal fibres supplied to the heart, when the researches of Von Bezold, in 1862, seemed to throw light upon this point. I have not repeated his vivisections, but he has done so himself a great number of times, and I think some importance may be attached to his conclusions. Von Bezold poisoned rabbits with *curare* to get rid of irritability of voluntary muscles; then, the sympathetic and pneumogastric nerves in the neck having been all carefully divided, artificial respiration was kept up. When the spinal cord had been divided at the seventh cervical vertebra, irritation of its distal cut end produced violent increase of the heart's movements, while irritation of its proximal end did not. This was one of a series of experiments which led him to the conclusion that the chief motor centre for the heart in relation to sensation and psychical impression, and one which contributes three-fourths of the motor impulse, is in the medulla oblongata. From this centre the fibres do not pass down to the heart through the *vagi* or sympathetics in the neck, but through the cord. Emerging from the cord near the upper part of the dorsal region, these nerves pass to the base of the heart, through the sympathetics, and with contributions possibly from ganglia below.

"Thus the anatomical nervous connexion between the heart and the region of infra-mammary pain became endowed with a great significance, and it seemed more than ever probable that the sensorium received the impression of *some distress* in the heart through nerves which enter the grey posterior columns of the cord at the same point as those from the sixth, seventh, and eighth intercostal

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spaces. The central impression is radiated, and referred by the mind to the sensitive skin, according to the laws of reflection.

"In the case of aneurism of the descending portion of the aortic arch, the same nervous circuit accounts for the pain in the side, which is indeed a more aggravated form of infra-mammary pain. In severe instances of intercostal neuralgia, painful spots are also to be found near the sternum and spine, at peripheral ends of cutaneous nerves, and even in the arm, which, through intercosto-humeral branches, receives also sensory nerves from the same source. In these and other ways, I think the theory here laid down explains the actual phenomena. For instance, the pain may be (though always to a much less extent) felt on the right side also. Now, although the first internal dorsal branches of the sympathetic on the right side go to the oesophagus, some of them do run on from it, and under it, to the aortic arch. Again, though the sixth, seventh, and eighth intercostal spaces form the habitat of this pain, it *may* be felt in the fifth and in the ninth also, this being readily accounted for by the delicate and very irregular filaments which run from the first and second dorsal ganglia, as well as from the great variations in the mode by which the dorsal sympathetics form the great splanchnic. So far, then, I believe there is some reason, on anatomical, physiological, experimental, and pathological grounds, for adopting this theory: that *infra-mammary pain is a reflex neuralgia expressive of some distress in the heart.*

"In our present extremely limited knowledge of cardiac innervation, I am scarcely prepared to support strongly any further conjectures as to what the exact nature of this cardiac distress may be. My own view is that it belongs to the cases of partial reflex paralysis, and that this is the reason why general muscular debility is its concomitant. Although the heart possesses nerve-centres of its own, we have seen that there is some ground for thinking that its action is regulated by the vaso-motor system through the cervical sympathetic and the vagi, while the great direct motor stimulus of mental and sensational acts, goes to it from the medulla oblongata. If the proposition be true, that (as Brown-Séquard tersely puts it,) 'almost all parts of the body may be affected with paralysis in consequence of an outside excitation,' why should not the heart be placed under a list which includes the 'arm, hand, face, eyes, neck, trunk, pharynx, oesophagus, bladder, &c.?

"Let me illustrate this hypothesis by an example. Nothing is more common than infra-mammary pain on exertion, in cases of leucorrhœa. Now, no one denies the great influence on the nervous system of irritation in the cervix uteri or neighbouring parts, these effects extending to the encephalon itself. There is nervo-muscular fatigue of the whole system, and the pulse becomes irregular as well as quick, especially when infra-mammary pain is, or has been present. But a quick pulse here means a weak heart, unable to produce sufficient propulsion in the normal time. This weakness is not defective nutrition, for it occurs frequently in the well-fed and luxurious. May it not be a neurosis expressing the exhaustion of a centre (in the medulla oblongata) by continued irritation (uterine

nerve)? Dr. Fuller's case, as another type, would be explained by a similar temporary exhaustion of the nerve-centre for cardiac action by a worried brain, the muscular build of the heart perhaps predisposing it to be overworked.

"In conclusion, as to the treatment which we should expect to answer if our theory be true: this must *remove the source of irritation, relieve the heart of its overwork, and allay the pain*. Now, to allay the pain for a time is not difficult; counter-irritants or simple plasters will do so, blisters, small and numerous, have been much used, and I have been satisfied with aconite and belladonna, especially the admirable liniments of the new *Pharmacopœia*. The next indication is to relieve the heart, for which, of course, repose in the horizontal position answers best, for the time causing the pain to disappear. The Turkish bath, for obvious reasons, acts in this direction. But the great point is to seek for a permanent cause; for though much may be done by direct spinal stimulants, as strychnia or cold bathing, &c., no ultimate good will result, except exhausting nerve-irritation be removed. Cure the gastric or uterine irritation, the leucorrhœa or the hemorrhoids, give repose to the fatigued spinal cord or worried brain; and, *pari passu*, as the heart's action improves, and is no longer too slow in repose, and too quick during muscular or mental exertion, so will there result a corresponding diminution of the infra-mammary pain."

ART. 39.—*Explanation of the Various Relations of Palsied Muscles to the Constant and Induced Current.*

By DR. E. NEUMANN KÖNIGSBORG.

(*Deutsche Klinik*, No. 7, 1864; and *Schmidt's Jahrb.*, No. 5, 1864.)

It is a fact deserving further inquiry, that in certain paralyses, especially of the face, the palsied muscles soon cease to react with the induction current, whether applied to the muscles themselves or to the nerves, while they contract very strongly, even more strongly than in health, upon the application of the constant current, that is to say, at the entrance or exit of the latter. In proportion as voluntary motor powers return, reaction with the induction current increases, while that with the constant current diminishes. These surprising phenomena might be due to three causes.—1. The rapid alternation of variously directed currents; 2. the varying direction itself; and 3, the momentary duration of the induction current. Comparative observations in a case of rheumatic facial palsy in which, at the end of a month all voluntary power and all reaction with the induction current had completely disappeared, while the constant current acted most powerfully, proved that the first two of these causes were without any influence in causing the difference; for both single induction shocks, and shocks at opening and closing circuit were without effect. The different duration of the currents is thus obviously the cause; for if the constant stream be artificially

rendered as nearly as possible momentary, contraction would fail to follow it, while so long as it continues regularly, even though in small quantity, the phenomena are constant.

ART. 40.—*Loss (Hysterical) of Speech and Hearing
successfully treated by the Inhalation of Ether.*

By Dr. JAMES H. HUTCHINSON, Physician to the Episcopal Hospital.

(*American Quarterly Journal of Medical Science*, April, 1864.)

CASE.—C. C., a German girl, aged twenty years, was admitted to the hospital September 22nd, on account of entire loss of speech, which had occurred ten days previously. Complete deafness supervened two weeks after admission.

She had always had good health up to two and a-half years ago, when she was about to leave Germany for this country. At that time she had a violent quarrel with her fellow travellers, which gave rise to a haemorrhage from the lungs. During the voyage she had several convulsions; but upon her arrival in this country she was sufficiently well to take the place of cook in a private family, and continued in good health, with short interruptions, up to September of last year.

I found her in the wards when I took charge of them at the beginning of the year. I learned that she had had but few convulsions while in the hospital, and that the usual remedies had been tried without relieving her condition. Electricity, the various tonics, nux vomica, and, lastly, sulphate of anilin, had all been resorted to without benefit. During all this time her only means of communicating with others was by means of a slate.

I was anxious to discover whether the loss of speech and of hearing was in any degree feigned, and with that view directed the resident physician to administer ether to her by inhalation, believing that if such were the case she would utter sounds, if not words, as she came under the influence of the anaesthetic. The result, however, was different from what I had expected.

The ether threw her into a slight convulsion, from which she soon recovered, having entirely regained her hearing.

Two days subsequently I directed the experiment to be repeated. Upon recovering her consciousness she was able to say "mamma," and to make various inarticulate sounds.

She was a third time (three days later) placed under the influence of ether, with the following curious result: that of recovery of speech and loss of hearing.

A fourth etherization produced no immediate result; but in the course of a few days she was able first to hear loud noises, and later to understand what was said when spoken in a loud tone of voice. The improvement after this was very rapid, and by the middle of January her recovery was complete.

She was retained until the beginning of February for observation; but as she continued quite well she was discharged at this time at her own request.

Her joy at her recovery may be well imagined, as all hope had been abandoned by herself and friends, by whom application had been made to the managers of the Deaf and Dumb Asylum for her admission to that institution.

ART. 41.—*Case illustrating the History of Loss of Speech in Cases of Hemiplegia.*

By Mr. W. M. H. WELBY.

(*Medical Times and Gazette*, July 9, 1864.)

The following case, which was under the care of Dr. Stewart, at the Middlesex Hospital, is recorded in the Weekly Reports of Hospital Practice in Medicine and Surgery. It is of much value as an evidence of disease of the right side of the brain with no loss of speech, and disease on the left with loss of that faculty. It was some time ago pointed out by M. Broca that disease on the left side of the brain was more frequently associated with loss of speech.

Dr. Hughlings Jackson states that he has now seen in his practice and in that of his colleagues, Dr. Ramskill and Dr. Radcliffe, at the Hospital for Epilepsy and Paralysis, forty cases of loss of speech with hemiplegia on the right side, and but one with hemiplegia on the left. In many of them there was valvular disease of the heart. This is a mere statement of facts, and does not imply any conclusion as to which side of the brain is the seat of the faculty of speech.

In this case, however, there was no loss of speech in the attack the patient had before admission; but in the first after admission he had hemiplegia on the left side, and no loss of speech, and then hemiplegia on the right, with increase of paralysis on the left, and loss of speech. Both middle cerebral arteries were found plugged.

Of course, no one can affirm that hemiplegia on the right side is even generally attended by loss of speech, but that when with loss of speech there is hemiplegia, the hemiplegia is nearly always on the right side.

CASE.—*Valvular disease of the heart—Hemiplegia on the left side, with no loss of speech—Hemiplegia on the right, with loss of speech—Death—Autopsy.*

J. C. (admitted into the Middlesex Hospital on August 14, 1862, under the care of Dr. A. P. Stewart), aged twenty-one, a jeweller, naturally pale, with dark-brown hair, hazel eyes, and long dark eyelashes. Father died of phthisis. When ten years old, patient had chorea, which commenced on the left side, caused by a fright. Recovered under medical treatment in six weeks. Had syphilis when eighteen; rheumatic fever when nineteen.

History of Illness.—Early in the evening of August 13, he was suddenly attacked with choreic movements of left arm and leg, which continued for several hours. Next morning he found that his right limbs were powerless. Sight, speech, and hearing unaffected throughout attack; no nausea, vomiting, nor vertigo. Rigor three weeks ago, since which he has felt languid and depressed.

State on Admission.—Quite conscious; face drawn slightly to right side; complete loss of power of left arm and leg, sensation however not quite destroyed; says affected side and limbs feel very numb; slight headache over right brow; pupils large, equal, and act equally to light; tongue furred, protruded to left of mesial line, sensation of tongue more complete on right than on left side; swallows without difficulty; cardiac dulness

greatly increased, vertically five and a half inches, transversely four and a quarter inches; powerful impulse of apex at lower border of sixth rib; intense and harsh double murmur heard from immediately below clavicle to apex, and also to outside of nipple; pulse 100, full, labouring, and with very perceptible backward stroke; visible pulsation of almost all arteries of body; breath-sounds normal. Ordered two grains of calomel and three of extract of henbane every six hours.

15th.—Slept tolerably, perspiring freely. Cannot move left arm, but can make slight pressure with fingers; can move left leg very slightly, but complains of pain extending up leg to side on so doing; feels pain when pinched on left side or limbs; articulates with tolerable distinctness, and swallows without difficulty; tongue as before; pupils large, equal, and sluggish; pulse 96; urine acid, sp. gr. 1020, no albumen.

Treatment.—To be cupped behind right ear to amount of four ounces. Ordered one drop of croton oil in mucilage directly, and ten grains of nitrate of potash in peppermint-water every six hours.

18th.—Traction of mouth to right side rather less; returning power of left arm and leg, but pains on moving the limb still severe; tongue as before; bowels freely open; pulse 96; heart-sounds continue the same; headache over right brow to-day disappeared; pupils unchanged. To continue draught.

20th.—Is gradually progressing, though still very weak. Ordered three ounces of port daily, and half a drachm of the syrup of the iodide of iron three times a day after meals. To have a chop daily.

September 19.—For last three weeks patient has been able to walk about the wards with the assistance of a stick; sleeps well; appetite fair; has lost flesh and is very pale; complains only of pain in right knee, which has a natural appearance. To continue as before; opium plaster to knee.

20th.—To-day walked round garden without assistance.

21st.—The nurse, finding from another that he was again suddenly paralysed, with loss of speech, called the clinical assistant, when the following symptoms were noted:—4 A.M.—Lying on back, and perspiring freely; expression anxious; complete loss of power and sensibility of left side; cannot protrude tongue, or speak, but is quite conscious, and can make an attempt to speak when addressed, pointing to region of heart when asked if he had pain anywhere; swallows with difficulty; hearing unaffected; pupils very large, equal, and act equally to light; no drawing of mouth to either side. Omit draught. To have two grains of calomel directly; cold lotions to brow. 11 A.M.—Still conscious. He wrote a few words with right hand in pencil, asking to see friends, paper being held for him by another patient. 2 P.M.—Fit, attended with convulsive movements of all limbs; passed urine involuntarily during attack; pupils still rather large, and act feebly to light; seems feeble, but conscious; pulse 130; heart acting with violence; sounds as on admission; cannot swallow. Head to be shaved; blister to crown. Enemata of beef-tea, with 5*j.* of wine every four hours. 4 A.M.—Pulse 100; pupils almost fully dilated, and act very feebly to light; distinct loss of power and sensibility of right side; left side as before. 10 P.M.—Still conscious; occasionally puts both arms out of bed, and can draw up legs when soles of feet are tickled; pulse 102; pupils as before.

22nd, 10 A.M.—Has had convulsive movements of limbs, especially hands, at intervals during night; pupils small, but sensible. 10.15.—Pupils larger; sensible. 1 P.M.—Unconscious; breathing stertorous; left pupil large and right pupil much smaller—act feebly to light; frequent convulsive movements. 2.30 P.M.—Pulse 96, thrilling; quite unconscious; breathing stertorous; respiration 18; irregular interval between breaths; eyelids

semi-closed ; pupils equal, moderately dilated, insensible to light ; all arteries of body pulsate strongly, except those of right leg below knee-joint, which pulsate very feebly ; motions and urine passed in bed ; sordes on lips and teeth. 4 P.M.—Died.

23rd.—*Autopsy Fourteen Hours after Death.*—Brain : Both middle cerebral arteries are filled with fibrinous plugs and semi-coagulated blood. At each extremity of the Sylvian fissures is a mass of diffused brain-substance about the size of a walnut. On the right side the softening extended into the corresponding lateral ventricle. No appearance of any old blood-clot. The right lateral ventricle is completely filled with coagulated blood, which is also found in a lesser quantity in left lateral and in fourth and fifth ventricles, a large clot pressing on medulla oblongata at base of brain. No other arteries plugged. Arachnoid membrane thickened and opaque, especially over sulci of brain. Thorax : Lungs healthy ; heart weighs $\frac{5}{3}$ xxj. ; walls of ventricles much hypertrophied ; both aortic and mitral valves were covered with fibrinous vegetations, both on free edges and curtains of valves ; neither orifice much contracted ; some vegetations also on tricuspid valves. Abdomen : Pericardium non-adherent, contains an ounce of clear serum ; spleen weighs $\frac{5}{3}$ xix. ; in lower part is a conical mass, about the size of a horse-chesnut, of cheesy consistence, presenting under the microscope abundance of oil-globules and granular matter ; right kidney also contains a similar deposit, size of a pea ; other viscera present no unusual appearance ; right anterior tibial artery contained several small clots of blood.

ART. 42.—*Spontaneous Electrical Discharge in Diseased Nerves.*

By Dr. TH. CLEMENTS.

(*Deutsche Klinik*, No. 3, 1864 ; and *Schmidt's Jahrb.*, No. 5, 1863.)

Clemens relates two cases in which "spontaneous discharges," similar to those of electricity, removed pain and convulsion. In the first, a rheumatic sciatica of great severity temporarily disappeared after a strong electric shock in the course of the sciatic nerve, which caused severe pain in the lower part of the thigh. In the second case, a traumatic tetanus, arising from a wound by a nail running into the sole of the foot, recurred in very severe paroxysms which regularly finished with tremors of great violence, passing from the spinal cord to the periphery. Clemens remarks, in connexion with these facts, on the great utility of strong shocks from a Leyden jar applied to the diseased nerves, in many cases of recent rheumatic and paralytic contraction.

(B) CONCERNING DISEASES OF THE RESPIRATORY SYSTEM.

ART. 43.—*On the Influence of Pleurisy in the Development of Phthisis.*

By M. BEAU.

(*Journ. de Méd. et Chir. Prat.*, Juillet, 1864.)

In the Hospital of La Charité is a patient whose case, though apparently of little importance, has given Dr. Beau the opportunity

of pointing out the influence which pleurisy seems to exert on the development of phthisis. Between these diseases there is a very close connexion ; often pleurisy merely supervenes upon phthisis ; but it is not uncommon to see a pleurisy occur in a subject who, till then, has presented no rational sign of phthisis, and to see it followed by the development of that disease. This was the opinion of Broussais, who attributed to inflammation the formation of tubercles, and Dr. Beau has met with many facts which have led him to the same conclusion. Thus, in the case of the patient in question nothing indicated a year ago that he was tubercular. This winter he took a pleurisy, perhaps two ; when admitted into La Charité there was still a little effusion into the left side, which remained persistent, and was accompanied with a little febrile excitement towards evening. A blister removed the effusion, but did not lead to the disappearance of the fever ; the patient was then carefully auscultated, and the presence of tubercle was recognised in the left inferior scapular region. Cases of the kind are not uncommon. Dr. Beau had for his house physician a young man who contracted pleurisy ; two years later he died tubercular. At this very time he has under treatment a patient whom two years ago he treated for pleurisy, and who is now tubercular. Many other examples could be quoted of tubercle supervening upon pleurisy. Is this any reason for treating these patients according to the system of Broussais ? By no means : a very spare diet is bad because it debilitates, and an enfeebled state of the organism opens the door to all the diseases which afflict humanity.

ART. 44.—*On the Therapeutics of Consumption.*

By DR. COTTON, Physician to the Hospital for Consumption at Brompton, &c.

(*British Medical Journal*, Nov. 25, 1864.)

In this paper Dr. Cotton gives the comparative results of a series of therapeutical experiments on which he has been engaged during the last five years. Each substance mentioned was given to twenty-five patients whose condition was as much alike as possible. Why cod-liver oil does not figure as one of these substances does not appear. Dr. Cotton proceeds :—

" *Phosphorus* in many cases proved a good tonic and stimulant, increasing both the appetite and strength of the patients. In a few instances, however, it obviously disagreed, producing nausea, a distaste for food, and abdominal derangement. Four of the patients (three of whom were in the first, and one in the last stage of the disease) left the hospital materially improved in every respect, and apparently with their general health restored ; but several of those who derived no benefit from the phosphorus improved afterwards under other treatment. It was administered according to the formula of the Prussian *Pharmacopœia*, in which four grains of phosphorus are held in solution by an ounce of olive oil. Of this

solution, from five to eight minims were given in a little mucilage twice or three times a day.. Five minims contained one-twenty-fourth of a grain of phosphorus.

"*Liquor Potassæ*, given in doses of fifteen minims two or three times a day, altogether failed to support its ancient reputation as a "deobstruent" in phthisis. It seemed generally to be inoperative; and a considerable number of patients, in whom it had produced no visible effects, improved afterwards under different treatment.

"*Hydrochloric Acid*, on the other hand, was productive of very marked benefit. In doses of ten or fifteen minims of the dilute acid two or three times a day, it generally increased the appetite, and lessened many of the urgent symptoms, especially excessive perspiration and pulmonary secretion. Under its continued use for several weeks, many of the patients materially improved.

"Although in certain dyspeptic conditions, the alkalies, in various combinations, are oftentimes of very great use in the treatment of phthisis, it appeared that the acids *separately* are far better suited to the so-called tubercular 'crasis' than the alkalies *separately*. Several patients who had made no progress under the liquor potassæ improved very much when this was changed for hydrochloric acid; and four who had been taking the acid with advantage, but to whom liquor potassæ was given as a change, urgently begged to return to the acid. In a very few instances did the hydrochloric acid disagree; now and then it appeared to cause a little gastric pain, but it was never necessary permanently to abandon its use. I have reason to believe that the other mineral acids will be found as effective as the hydrochloric. The circumstance, however, that the free acid frequently occurring during healthy digestion is the hydrochloric, and that this acid is the well-known solvent of the plastic constituents of the food, made me select it in the present experiments; but I have often prescribed both the nitric and the nitro-hydrochloric acids in the wards of the hospital at Brompton with the same good result.

"*Iodide of Iron*, in doses of a drachm of the syrup two or three times a day, appeared greatly to benefit several of the very early cases, and was particularly serviceable in some which exhibited evidence of chronic secondary pneumonia. In a few instances it was discontinued, on account of its producing headache, with dyspeptic symptoms; but as a general rule, it seemed to agree with the patients. Under its influence, many gained materially in weight.

"*Iodide of Potassium*, which was at one time held in great repute in the treatment of phthisis, seemed remarkably ineffective in most of the twenty-five cases in which it was employed. Occasionally it gave rise to some dyspepsia, but more commonly its administration was unattended by any decided result. Only six cases materially improved during its use; and, in nearly every instance in which it was given, the patient's weight sensibly diminished.

"*Chloride of Sodium* was prescribed in doses of from one to three drachms dissolved in water two or three times a day. It was observed that one drachm could generally be taken without pro-

ducing nausea ; that, in a few instances, two drachms caused a slight degree of sickness ; whilst three drachms sometimes gave rise to actual vomiting ; although, as a general rule, these larger doses, when gradually arrived at, did not disagree with the stomach. A larger dose could be taken after meals than when the stomach was empty. In only three cases out of the twenty-five was thirst complained of. In this experiment, a fair number of patients increased in strength and appetite, justifying the conclusion that, in many such cases, common salt deserves to rank as a tonic.

"A large amount of chlorides was always found in the urine of those who were taking the salt ; but so little in relation to the quantity administered, that much must either have passed off by the bowels, or remained in the system. The search after chlorides led to the discovery that the urine of all consumptive patients, even of those in the last stage of the disease, contains such compounds in considerable quantity.

"*Steel Wine* proved a most useful auxiliary in the treatment of a considerable number of patients. It seldom disagreed, but tended rather to increase the appetite and improve digestion. In the cases of children and young persons, especially when combined with cod-liver oil, it was remarkably efficacious. The dose varied from two drachms to half an ounce, and in a few cases to an ounce, once or twice a day. From half an ounce to an ounce of the vinum ferri, taken immediately after dinner, is, I believe, one of the best means of administering steel in phthisis. In this way, it seems to interfere less with the function of digestion, and to be more easily absorbed into the system.

"A well-known French author, M. Troussseau, has recently asserted his belief that steel, so far from benefiting, not unfrequently engenders phthisis. Has he not here failed in the distinction between the *post* and *propter hoc*? Unquestionably, many become consumptive in spite of all the steel we can get them to take ; but, if consumption were connected with steel-taking, assuredly this disease would be even more common than it is !

"*Glycerine* has been much extolled by some writers as a remedy for phthisis. I gave it in doses of from one to two and occasionally three drachms twice and sometimes three times a day. In five of the twenty-five cases, it either caused sickness or otherwise disagreed. Sometimes it seemed to relieve the cough ; but few of the patients derived any material benefit from its use, and very few of them gained anything in weight whilst taking it. Several, who made no progress under the glycerine, improved subsequently under cod-liver oil. Many observations were made in reference to the comparative usefulness of these two agents, the result being invariably and very considerably in favour of the oil. In ordinary cases of phthisis, indeed, I believe glycerine to be of little use.

"*Sesquichloride of Iron* conduced to some very happy results. A large number of cases improved under its use ; and six patients in whom the disease was decidedly established, after having taken it for several weeks, left the hospital with their health restored, and in a condition to resume their respective occupations. Were I to be asked which of the twelve remedies I had found most generally

adapted to the ordinary run of consumptive cases, I should say the sesquichloride of iron. For several years I have been in the habit of prescribing it in doses of from ten to fifteen minims twice a day; and the mixture of the sesquichloride of iron is one of the most frequently employed formulæ of the Consumption Hospital *Pharmacopeia*. In private practice, it may be agreeably taken in combination with lemon or orange syrup; and in this way it may be continued with advantage for many weeks, and even months.

"*Chlorate of Potassa*, like glycerine, has won various opinions as to its influence upon consumption. By one physician it has lately been considered even as a *specific*, whilst by others it has been deemed altogether a failure. After a careful analysis of its effects in the twenty-five cases, I came to the conclusion that it was very far from a *specific*; and that its usefulness was seldom apparent, except in those cachectic cases in which it and allied remedies are frequently serviceable.

"*Quinine*, in doses of one or two grains twice and sometimes three times a day, although well suited apparently to a few of the twenty-five cases, generally produced disappointment; its effects being, as a rule, inferior to those of many other tonics. It would seem that quinine, like chlorate of potass, is chiefly useful in a certain class of cachectic patients, in which, irrespective of the tubercular condition, such agents are very generally prescribed. Several of the patients, however, who had scarcely benefited by the quinine alone, improved subsequently under the combined influence of quinine and iron.

"*Phosphoric Acid* was prescribed in doses of fifteen minims two or three times a day. In three or four cases, it seemed to produce pain in the bowels, with nausea and loss of appetite; although, as a general rule, it agreed very well. Only three cases improved materially under its influence; whilst several of those in whom there was a moderate improvement benefited much more under other treatment. Phosphoric acid may be classed amongst the mineral acids as a general tonic in phthisis, but seems to me inferior to some of them, especially the *hydrochloric*.

"The following table shows the general effect of these various agents upon the twenty-five cases in which they were administered:—

	Considerably improved.	Slightly improved.	Not improved.
1. Phosphorus	4	5	16
2. Liquor Potassæ	1	2	22
3. Hydrochloric Acid	11	6	8
4. Iodide of Iron	10	4	11
5. Iodide of Potassium.....	6	5	14
6. Chloride of Sodium	8	6	11
7. Vinum Ferri.....	13	3	9
8. Glycerine	4	2	19
9. Sesquichloride of Iron	12	5	8
10. Chlorate of Potassa	5	4	16
11. Quinine	7	5	13
12. Phosphoric Acid	3	8	14

"It must not be forgotten, in estimating the therapeutic value of these different substances, that this is illustrated by the table rather *comparatively* than *absolutely*; since we must not exclude from its proper share in the result the combined influence of hope, rest, good diet, and general hygiene, under which, even by itself, many phthisical cases are well known to undergo very considerable improvement. The figures, indeed, can only be taken as a fair expression of the *comparative* usefulness of the various agents as therapeutical auxiliaries to general treatment.

"The experiments to which I have thus briefly alluded, and which will be found more fully described in my papers in the *Medical Times and Gazette*, appear to be suggestive of the following general conclusions.

"1. Since, during the administration of each one of the agents described above, several cases of phthisis were observed to run through the various phases of the disease, some even to a fatal termination, it is obvious that, whatever the amount of benefit which in some cases followed their use, no one of such agents deserves the title of '*specific*'.

"2. It may, therefore, fairly be concluded that the good effected by any of these agents was due to their respective tonic and upholding influence upon the general system.

"3. In the majority of phthisical cases, steel (especially the sesquichloride of iron) and the mineral acids appear to be most effective; but tonics generally are productive of more or less improvement.

"4. Since, however, even steel and mineral acids, as well as other useful tonics, are undoubtedly inert in a certain proportion of cases, it is not improbable that there are varieties or modifications of phthisis, each of which may require a particular treatment. As there are special varieties of many other diseases, requiring special modes of treatment, and yielding to none other, it is possibly the same with consumption.

"I would observe, in conclusion, that I think we are too apt to consider and to treat phthisis as a separate and always similar disease, disregarding the almost endless varieties it presents. Whether such varieties are dependent upon original differences in the nature of the disease itself, or are determined by peculiarities in the individuals it may attack, we at least have before us a large field for experimental therapeutics. My own idea is, that the time is not distant when observation and experiment will show that, under certain at present obscure conditions of system, phthisis assumes definite and special forms, each of which requires definite and special management; and that the 'therapeutics of phthisis,' which many may now consider both unsatisfactory and unpromising, will contribute, some day or other, to great and unexpected results. I am not so sanguine as to look for a *specific* in consumption; but that the disease will ultimately prove as amenable as many others to proper management, is, I hope, not a mere day-dream."

ART. 45.—*On the Assimilation of Fat in Consumption.*

By Dr. HORACE DOBELL, Physician to the Royal Infirmary for Diseases of the Chest.

(*Lancet*, September 10, 1864.)

In the first place, Dr. Dobell directs attention to the fact that, as a general rule, persons suffering from consumption dislike fat; in the next place, he proceeds to test, by direct experiments, whether this dislike for fat is due to a defect in the emulsifying properties of the pancreatic secretion. In order to this,—

"I determined," says Dr. Dobell, "to treat a series of cases of consumption at the Royal Infirmary with pancreatic juice artificially introduced into the digestive apparatus.

"A number of practical difficulties presented themselves, many of which will be familiar to those who have experimented with pancreatic juice even on a small scale; but these were materially increased by the circumstance of having to keep up a constant supply to a number of patients, and by the patients being treated out of hospital, and, therefore, requiring to take home with them the quantity of medicament necessary to last till their next visit to the infirmary. Many different experiments were made before a plan was fixed upon which appeared practicable; and I cannot too warmly praise the care, skill, and intelligence of Mr. Heathorn, a most promising young chemist in the establishment of Mr. Balmer, of St. John's-street-road, who assisted me in overcoming these difficulties, and afterwards prepared the pancreatic juice all the time it was used at the infirmary.

"The plan we finally decided to follow was, to prepare an emulsion of beef-fat with the pancreatic juice of the pig, of the consistence of thick Devonshire cream; to supply the patient with this emulsion in a covered jar holding a week's allowance, and to order each dose to be taken stirred in milk. The emulsion mixed with the milk without any difficulty, and in the proportion of half an ounce in a breakfast-cupful of milk was not at all an unpleasant drink.

"Comparative specimens were prepared of cod oil and of beef fat, emulsified with liquor potassæ and with pancreatic juice, and submitted to microscopical examination.

"The following report was made by my friend Mr. Farrants on specimens submitted to him.

"Microscopical Examination of Emulsions of Cod-liver Oil and Beef Fat with Liq. Potassæ and with Pancreatic Juice."

DIAMETERS OF GLOBULES.

Cod-liver Oil and Liq. Potassæ :

a Largest $\frac{1}{800}$ " ·0012" Not numerous.

β Commonest ... $\frac{1}{3600}$ " ·0003" Very common.

γ Smallest $\frac{1}{10000}$ " ·0001" As numerous as preceding.

Emulsion thin, readily separating into two layers.

Cod-liver Oil and Pancreatic Juice:

- α Largest $\frac{1}{300}$ " ·0030" In considerable number.
- β Commonest ... $\frac{1}{1250}$ " ·0008" Common.
- γ Smallest $\frac{1}{16000}$ " ·00006"..... Abundant.

In this emulsion larger globules occur, and are more common than in the first. The smallest met with are smaller and in much greater number than in the potash emulsion. The oil is more minutely divided; the emulsion is thicker and more permanent.

Beef Fat and Liq. Potassæ:

- α Largest $\frac{1}{1600}$ " ·0006"
- β Commonest ... $\frac{1}{2500}$ " ·0004"
- γ Smallest $\frac{1}{16000}$ " ·0001"

But few clearly defined globules distinguishable. The fat is almost always entirely saponified.

Beef Fat and Pancreatic Juice:

- α Largest $\frac{1}{1400}$ " ·0007" Not numerous.
- β Commonest ... $\frac{1}{2500}$ " ·0004" Far most common.
- γ Smallest $\frac{1}{16000}$ " ·0001" Uncommon.

Emulsion fairly uniform. No tendency to separate.

"The liq. potassæ emulsions differed greatly from those with pancreatic juice, the latter being much more complete and permanent emulsions, the former being more or less complete soaps. The smallest globules seen were those of cod-liver oil and pancreatic juice—viz., $\frac{1}{16000}$ of an inch, which were abundant. This would probably have been the best for my experiments; but I selected the beef fat emulsion in preference, in order that there should be no confusion between the ordinary effects of cod-liver oil and the effects of the pancreatic juice.

"The beef fat emulsion remained complete after standing two days, mixed with pepsine, hydrochloric acid, and water.

"As I do not propose on this occasion to enter into a discussion of the facts produced, but simply to submit them to the consideration of others, it will not be necessary to make any further introductory remarks, except to mention, in explanation of some things that may be noticed in the following analyses of the cases—1st, that the experiments were made upon out-patients; 2nd, that an out-patient's letter at the Royal Infirmary only lasts eight weeks. Those who have attempted to keep notes of cases amongst outpatients know how difficult it is to do so satisfactorily; how much trouble is often thrown away, by patients, who think they are getting well, discharging themselves without giving the physician the opportunity of making the final report necessary to show the progress of the case. It often happened so in my cases, and they had to be discarded from the tables, although it will be seen that I have retained a few in which all the points were filled up except the last. Some cases had to be rejected after they were completed, in consequence of facts turning up in their course which made it doubtful whether the original diagnosis of consumption was un-

questionably correct. With these exceptions, no selection was exercised; the cases were taken for experiment in the order in which they applied for admission, and the particulars registered under the following principal headings:—1, Name; 2, occupation; 3, age; 4, sex; 5, condition on admission (*a*, general symptoms; *b*, physical signs); 6, condition on discharge (*a*, general symptoms; *b*, physical signs); 7, quantity of emulsion taken per day; 8, how long continued; 9, effects; 10, effects of cod-liver oil; 11, other treatment; 12, notes.

"The results of treatment in the 33 cases retained for analysis may be seen in the following—

"Summary of 33 cases of Consumption treated with Pancreatic Emulsion of Beef Fat:—

Seven cases in the 1st stage—Condition on discharge measured by the general symptoms: All improved.

Condition on discharge measured by physical signs: All stationary.

Emulsion agreed, 6; emulsion disagreed, 1.

Cod-liver oil agreed, 3; disagreed, 1; not tried, 3.

Fourteen cases in the 2nd stage—Condition on discharge measured by the general symptoms: Improved, 9; stationary, 2; worse, 1; no final report, 2.

Condition on discharge measured by physical signs: Improved, 8; stationary, 2; worse, 2; no final report, 2.

Emulsion agreed, 14; emulsion disagreed, 0.

Cod-liver oil agreed, 5; disagreed, 5; not tried, 4.

Twelve cases in the 3rd stage—Condition on discharge measured by the general symptoms: Improved, 8; stationary, 0; worse, 3; no final report, 1.

Condition on discharge measured by physical signs: Improved, 5; stationary, 1; worse, 3; no final report, 3.

Emulsion agreed, 10; emulsion disagreed, 2.

Cod-liver oil agreed, 5; disagreed, 5; not tried, 2.

The whole 33 cases measured by general symptoms: Improved, 24; stationary, 2; worse, 4; not noted, 3.

The whole 33 cases measured by physical signs: Improved, 13; stationary, 10; worse, 5; not noted, 5.

Emulsion: Agreed, 30; disagreed, 3.

Cod-liver oil: Agreed, 13; disagreed, 11; not tried, 9.

"Average quantity of emulsion taken by each patient, about 1 oz. avoirdupois in 1 pint of milk each day for eight weeks.

"The 33 patients consumed about 105 lbs. of emulsion, which required 315 pancreases to emulsify the fat."

**ART. 46.—Cases illustrating the Effects of the Pressure of
Morbid Growths upon the Pneumogastric Nerve or its
Branches.**

By Dr. HABERSHON, Assistant-Physician to Guy's Hospital.

(*Proceedings of the Royal Med.-Chir. Soc.*, Jan. 26, 1864.)

After referring to the complex distribution of the pneumogastric nerve, and to the important symptoms of disease produced by the implication of its branches, the author proceeded to describe several cases of thoracic aneurism in which the branches of the recurrent laryngeal nerve, or the trunk of the pneumogastric, were involved.—The first case was that of a man, aged thirty-nine, who had been engaged in laborious work at Chatham and Woolwich dockyards. Two months before death he began to suffer from hoarseness, and the laryngeal symptoms were more marked than any other. There were paroxysms of urgent dyspnoea and slight dysphagia. Five days before death haemorrhage from rupture into the trachea commenced; and on the morning of his death the bleeding suddenly became profuse, and was quickly fatal. Dilatation of the aorta, double aneurism of the arteria innominata, and perforation into the trachea, were found. There was pressure upon the commencement of the recurrent nerve, and commencing degeneration of the muscular fibre of the laryngeal muscles on the same side. As to the physical signs, dulness and double bruit were produced immediately over the first bone of the sternum, but there was no bruit over the aortic valves. No pain had been complained of. The value of the laryngoscope had been shown in demonstrating that no disease of the larynx existed, slight oedema of the mucous membrane only being present.—The second case was that of a sailor, aged thirty-five, who had apoplexy, with aphonia and symptoms resembling phthisis; aneurism was, however, suspected. The recurrent and the pneumogastric nerves were both compressed: the muscles of the larynx were on one side pale and wasted, and the lung on the same side was in a state of asthenic pneumonia. The thoracic duct had also apparently been compressed.—In the third instance recorded, the symptoms of cardiac disease obscured those of aneurism. There had been pericarditis and endocarditis, and fibroid degeneration of the muscular fibre of the left ventricle existed. The patient was a groom, aged forty-five, and two months before death symptoms of catarrh and bronchitis came on; the heart's action was irregular and tumultuous; the pulse very feeble, but without bruit. Afterwards a triple sound was produced below the nipple, and with renewed bronchitis there were signs of pleuro-pneumonia of the lower lobe of the right lung. No pain and no dysphagia were complained of, but paroxysms of urgent dyspnoea, with extreme faintness; in one of these attacks he died. In addition to the degeneration of the heart, an aneurism was found at the commencement of the transverse arch; and the pneumogastric, after giving off its recurrent branch, passed directly over the sac, and the compression of this nerve had determined the pneu-

monic consolidation of the lower lobe of the right lung.—In the fourth case, the recurrent laryngeal nerve was compressed, and had produced paroxysms of urgent dyspnoea, but the dyspnoea was in great measure due to direct pressure upon the trachea. There had been feebleness of the voice, with dysphagia. Pain was of an agonizing character, but also paroxysmal, and it was apparently due to direct pressure on the nerves. The aneurism of the aorta was situated immediately beneath the subclavian, and had ultimately perforated the trachea; but rapid effusion of blood had been prevented by layers of fibrin, and it was probable that the first oozing of blood took place nine months before death. No bruit had been produced, but a ringing second sound; the heart was healthy. The author stated that pressure on the pneumogastric nerve and its branches by aneurismal tumours in the chest led, first, to paroxysmal and spasmodic contraction of the muscles of the larynx; secondly, to diminished muscular power, and to paralysis and wasting of the laryngeal muscles; and thirdly, to pulmonary congestion and consolidation; but that gastric symptoms, such as were found in peripheral pulmonary irritation of incipient phthisis, were not observed in thoracic aneurism. The effect of changed nervous supply of the oesophagus was briefly referred to; spasmodic contraction, and possibly also ulceration, taking place without direct pressure; the author stating that spasmodic contraction from this cause aggravated the effects of the direct pressure of tumours generally. It was likewise mentioned that occasionally no dysphagia existed, because the whole of the oesophagus opposed to the tumour was pushed aside *en masse*. In conclusion, the author detailed an instance of disease affecting the supra-renal capsule, with bronzed skin; and exhibited a drawing from his dissection of a branch of the pneumogastric to the capsule. The irritability of the stomach often present in these cases was referred to this connexion. A dissection showing the larger branches of the semi-lunar ganglia, and the manner in which some of these branches were involved in the diseased capsules was also exhibited.

ART. 47.—*On the Treatment of Whooping-Cough in Gasworks.*

By M. GUÉRARD and others.

(*Journ. de Méd. et Chir. Prat.*, Septembre, 1864.)

M. Guérard, a member of the Board of Health of the Département de la Seine, forwarded on this subject to the Medical Society of Hospitals a communication which gave rise to interesting remarks from MM. Blache, Barthez, Roger, Bergeron, Maingault, &c.

Coal-gas is deprived of the sulphuretted hydrogen and carbonic acid gases always present in the crude product, by being passed through iron vessels partly filled with sulphate of lime and hydrated sesquioxide of iron. When these substances have fulfilled their object, they are extracted from the apparatus, and exposed in thick

layers on the pavement of one of the yards of the works, and thus rendered fit to be employed again. When placed in contact with the atmosphere, they evolve a large quantity of ammonia, mixed with light volatile oils. These exhalations, much complained of by persons who reside in the neighbourhood of gas-works, have of late been highly extolled in the daily periodicals, and have become a popular remedy for whooping-cough. Any person who might be tempted to visit a gas-factory at certain hours might mistake it for the playground of a school; children affected with whooping-cough are reported to have promptly recovered after having accidentally passed a few hours in these yards; others were in consequence brought to the same place, in the hope of securing for them similar benefit, and now these courts are scarcely large enough to admit all the applicants for admission.

It was, therefore, extremely important to ascertain if the alleged efficacy of the emanations in question was real, whether the inhalation was harmless, and also to which of the gases and volatile oils the effects observed are referable. Several members took part in the debate.

M. Barthez stated that he had witnessed two cases illustrative of the utility of this mode of treatment. The patients were two sisters, aged respectively three years and a-half and five years and a-half, both suffering from whooping-cough, which had lasted a fortnight in the former, and three weeks in the latter. The parents, who had heard of the efficacy of these inhalations, sent the children every day regularly to the gas-works for some hours, and in both instances a complete cure was effected, in one of the cases three weeks, and in the other four weeks and a half after the first onset of the disease—a duration much shorter than that usually ascribed to whooping-cough. No conclusive inference can of course be drawn from two cases, and M.M. Blache, Bergeron, Maingault, and Roger brought forward others in which no improvement whatever was obtained, and some in which the treatment would seem to have aggravated the symptoms. M. Blache stated that two children of thirteen and fourteen years, belonging to the same family, and both in advanced stages of whooping-cough, were taken eight days in succession to the gas-works, and in both the paroxysms increased in violence, and were subsequently allayed by the usual sedatives. M. Blache also procured a tubful of the residue employed at the works, and caused the fumes to be inhaled in several cases of whooping-cough, but derived no benefit whatever from the experiment. M. Roger added that he had been called in consultation for a child suffering from pneumonia, caused by the inhalation of the gases evolved in the works, but observed that exposure to cold might possibly have contributed to the development of the affection; in two of his own patients, however, the treatment in question signally failed, and produced no improvement whatever after a fortnight. Desirous of forming an opinion as to the real utility of the remedial agent, M. Roger also obtained the residue left after the purification of gas, and caused it to be spread out on the floor of a large hall in the Hospital for Infancy, where children belonging to his own and

to M. Bouvier's wards, suffering from whooping-cough, were conveyed every day. The results of the experiment having proved entirely negative, M. Roger came to the conclusion that the new mode of treatment presented no serious advantage, and that mere change of air was far more efficacious. We have ourselves had an opportunity of judging of the effects of the inhalations in two children, aged respectively three years and four months, both affected with whooping-cough; they were conveyed to the gas-works, the eldest was promptly relieved, but the youngest died of extensive bronchial inflammation, although the most active counter-irritation was resorted to.

In conclusion, we may say that, up to the present day, the inhalation of the products evolved from the substances used in the purification of gas has not justified its reputation, and in some instances seems to have been highly injurious. M. Guérard, however, whose sagacity our readers are well acquainted with, conceives that further inquiry is necessary, and that it is not inadmissible that these gases, which consist merely of ammonia and volatile oils, may yet be found useful in whooping-cough; indeed, ammonia has most certainly been beneficial in certain fits of asthma; it is therefore desirable to ascertain under what circumstances the inhalations alluded to may be advantageous, and to discover some improved means of administering the remedy in such a manner as to obtain its full curative effects, and at the same time to avert the complications which observers have ascribed to its action.

AET. 48.—*On the Use of Bromide of Potassium in Coryza, Dysphagia, and Spasmodic Cough.*

By M. GUBLER.

(*Journ. de Méd. et Chir. Prat.*, Août, 1864.)

The anaesthetic effects of the bromide of potassium on the soft palate are now well-known to surgeons. M. Gubler conceives that this singular property might be taken advantage of in the treatment of various morbid conditions, and states that with this remedial agent he had succeeded in modifying in the most favourable manner coryza, angina, painful deglutition, and even spasmodic paroxysms of coughing, which occasion so much distress to persons labouring under laryngo-bronchi'is and pulmonary tuberculosis.

The following is M. Gubler's prescription:—

B. Potassii bromidi, 3ij.;
Aqua destill. 3v.

Distilled water must be used, and the mixture preserved in stoppered bottles; if this precaution is omitted, the solution becomes turbid and offensive. A table-spoonful morning and evening is the usual dose.

We shall revert on some future occasion to the therapeutic effects of the bromide in chest affections, but we will at present confine

our observations to the prompt relief afforded by this remedial agent in consumption, when, from ulceration of the fauces, deglutition has become so painful as to deter the patient from the use of food. M. Gubler relates the case of a Zouave affected with tuberculosis, who voluntarily abstained from taking nutriment in order to avoid the torture coincident with deglutition. Half a drachm of bromide of potassium was exhibited, and immediately improved his condition. He was, after twenty-four hours, enabled to swallow bread and chicken without pain, and survived for a twelvemonth. The bromide, in such cases, is therefore a valuable sedative, and will be found efficacious for the cure of glandular angina unconnected with tuberculosis, and caused by local irritation or exposure to cold, and also for herpetic and arthritic sore-throat. M. Gubler's researches also point to the beneficial effects of the drug in the second stage of uncomplicated inflammatory angina, when the contraction of the pharynx and fauces retains sufficient intensity to compel the patients to abstain from food, and when, therefore, long and tedious convalescence, if not exhausting disease, such as diffused asthenic paralysis, may be expected to follow.

[Bromide of potassium and also bromide of ammonia have been used by several observers in this country, with the same object in view, long before the publication of M. Gubler's paper.]

(c) CONCERNING THE CIRCULATORY SYSTEM.

ART. 49.—*On the Diagnosis of Adhesions of the Pericardium.*

By Professor FRIEDRICH, of Heidelberg.

(*Archiv für Path. Anat.*, t. xxix, 1864.)

Of all the signs by which general adhesions of the pericardium may be diagnosed with greater or less certainty, there is none which is acknowledged as pathognomonic. Retraction of the chest-wall on a level with the heart's apex, occurring during systole, is far from being an absolute sign, as once imagined; numerous cases have shown that it may occur, and yet the pericardium be perfectly healthy. It becomes of greater value, however, according to Professor Friedrich, if observed concurrently with another phenomenon never recorded in such cases before, namely, a jerking and sudden emptying of the jugular veins during the diastole of the ventricles. In two cases this phenomenon was observed during life, and after death complete adhesion of two layers of the pericardium was found. In both cases, there had been increase of the area of precordial dulness, and retraction of a large portion of the chest-wall during the ventricular systole. On the other hand, during the ventricular diastole, there was felt over the same space a powerful impulse coextensive with the second sound, and strong enough to heave up the head of the auscultator. During the ventricular systole, the subcutaneous veins of the neck were also seen to get considerably distended, whilst, simultaneously with the diastole impulse they

emptied themselves so completely as almost to become invisible. These phenomena were more marked during inspiration than during expiration.

The lower surface of the pericardium was in both cases also closely adherent to the diaphragm over a large extent, and by this fact Professor Friedrich chiefly explains the systolic retraction of the chest-wall. He believes that this retraction must occur whenever the heart is unable to move from above downwards, as it does normally during the contraction of the ventricles. As to the sudden emptying of the jugulars, it is only a consequence of the diastolic impulse of the chest-wall, which necessarily produces a sudden diminution of the intra-thoracic pressure. It should be remembered that this diastolic impulse of the chest-wall has been described by Dr. Potain, under the name of Choc Diastolique, in "Bulletins de la Société Anatomi." Août, 1856. It has also been mentioned by Skoda ("Abhandl. über Perk. und Ausc.," 5th Ed. Wien, 1854, p. 156), and by Cejka ("Prager Vierteljahrsschrift," xii. 2, 1855).

ART. 50.—*On the Fatty Degeneration of the Heart.*

By DR. ORMEROD, Physician to the Sussex County Hospital at Brighton.

(*Medical Times and Gazette*, August 6, 1864.)

In the excellent review of the present state of cardiac pathology, which review formed the Address in Medicine delivered at the last General Meeting of the British Medical Association, Dr. Ormerod makes the following remarks upon this subject:—

"It has been said that the decomposition of muscular tissue may be so regulated that the ultimate result shall be fatty matter; and it has been suggested that the transformation which takes place in the living body is the same as that which we can induce artificially by these means: in fact, that fatty degeneration is a physical, and not a physiological or pathological process.

"The experiments conducted for me by Mr. J. Peel, with all conceivable exactness, have not justified this view. I subjoin an account of these experiments in detail; for the present I will merely recapitulate the results at which we have arrived, and which seem incompatible with any purely chemical theory of fatty degeneration.

"We found the excess of fatty matter in a fatty heart to consist of oleine, not of margarine, which is the normal fatty constituent of the human heart. Adipocere was proved to be by no means chemically identical with animal substance which had undergone fatty degeneration. It was merely a soap of lime combined with the fatty acids pre-existing in the tissue. There was no new fatty acid made—as is the case in fatty degeneration—during the change into adipocere, no substitution of oleates for margarates; and, indeed, it seemed that we could prevent the formation of adipocere altogether by carefully excluding lime during the process. We submitted

weighed portions of muscle, in which the quantity of fatty matter had been previously determined, to more than one process for making adipocere, and carefully analysed the results. In no instance was the fatty matter found to have been increased during the operation. On these grounds I cannot doubt that adipocerous transformation is a saponification, not a degeneration ; and that fatty degeneration of the heart is a pathological result not attainable by any of these chemical processes.

" Commencing decomposition simulates very closely that change in the heart's muscular structure which is known as the first or granular stage of fatty degeneration. I have been deceived by it myself ; and such a mistake is all the more likely to occur, because granular degeneration, like decomposition, may involve the heart uniformly over a very wide extent ; much more widely than distinct fatty degeneration ever does, probably because the destruction of the muscular fibres is less complete. This same uniform appearance of granular degeneration is also highly characteristic of the effects of maceration in dilute spirit, on the muscular structure of the heart ; but it is not really fatty degeneration. Ether only clears these fibres of some of the granular dottings, it does not entirely remove this appearance, still less does it empty the sarcolemma. Further, chemical analysis of a portion of heart which has undergone this granular degeneration during life shows but a slight increase in the normal quantity of fatty matter. This change is the first step, both in the chemical transformation into adipocere, and in the pathological replacement of the muscular fibre by oily matter ; but it is not itself fatty degeneration. These dots are not oil-globules, but the signs either of an integral change, on which fatty degeneration is subsequently engrafted ; or, if so it should be, of commencing decomposition.

" The general conditions with which fatty degeneration of the heart is found to coincide are, for the most part, those with which we connect muscular atrophy and fatty degeneration of other organs. Such are phthisis, haemorrhage, debility from long mental and bodily suffering, and the effects of indolence or intemperance. The cases, however, with which we are here chiefly concerned are those where the heart does not so much share in the general decay, as the decay begins at the heart, where the wheel is broken at the very cistern, as it were. Sometimes, while all the functions are regularly performed, the heart alone of all the organs is undergoing this fatal change ; and the first intimation that anything has been going wrong is the sudden death of a seemingly healthy man. A gradually accumulating experience has, indeed, given us some general grounds for conjecturing the existence of fatty degeneration of the heart during life ; and sudden death from this cause rarely comes altogether without warning. Still the fact remains, that in this decay the heart will often outstrip all the other organs, and will fail from a form of disease against which one would have thought that its own functions, its own constant activity, were the best preservatives.

" We are not in a position to say precisely what is the cause of

this peculiar change originating in the heart's structure. We know, indeed, that the heart is liable to undergo fatty degeneration in consequence of inflammation extending inwards from the pericardium; but such an occurrence is rare. Indeed, the habitual absence of all traces of inflammation of the lining membrane of the heart, in cases of fatty degeneration—recollecting that this disease, as a rule, extends from within outwards,—negatives the idea of inflammation, in the common acceptation of the word, being its habitual cause.

" Yet we cannot entirely dismiss the idea of inflammation as a cause of fatty degeneration of the heart, though the process which leads to this result has little in common with that which we familiarly know as tending to the exudation of coagulable lymph or pus. The aneurismal pouches which we find in the ventricles seem to originate in such a process. We see only the ultimate results over the prominent part of the sac; but at the edges, where it adjoins the healthy muscular tissue, we gain some insight into the nature of the process by which these results are reached. And here the zone of intense congestion points to excessive vascular action, to give it no more exact name, as the cause of this change of structure. One such case occurred in a girl, fifteen years of age, who had been ill only one month, and had suffered from heart symptoms during only a fortnight. And here the congestion was most intense at the line of union of the diseased and healthy tissues. Whatever share, however, this excessive vascular action may have in the process, the real analogies of acute fatty degeneration of the heart are to be found, not in inflammation of any organ, but in yellow atrophy of the brain and yellow atrophy of the liver,—both essentially acute fatty degenerations.

" Perhaps few pathological principles have met with more prompt and general acceptance than fatty degeneration of the heart in all its bearings; for, while it offered a sufficient explanation of much that had before seemed perplexing, it contradicted no known and established doctrines. Subsequent clinical experience has confirmed and extended the earlier deductions, and extends them still; and if yet there is a feeling of disappointment because angina pectoris has not received a full explanation from our present knowledge of fatty degeneration of the heart, is not this partly because cases of angina pectoris have seemingly become more rare, from the greater precision which the knowledge of this anatomical change has introduced into the nomenclature of cardiac disease?"

ART. 51.—*On Heart Disease in the Army of the U.S. of America.*

By Dr. HENRY HARTSHORNE.

(*American Journal of Medical Science*, July, 1864.)

The following remarks are quoted from the Transactions of the College of Physicians of Philadelphia:—

" Among the chronic affections of soldiers, which are best studied

in hospitals remote from the field, is one which does not seem to have met, as yet, with full appreciation by medical officers, inspectors, and pension surgeons. If the view which I hold be correct, the subject is of enough importance to deserve careful consideration by all who are interested in the health of the army. The affection to which I allude may be designated as *muscular exhaustion of the heart*. Although examples of it occur in all our military hospitals, I am acquainted with but one distinct published recognition of it; in the address of Dr. A. Stillé, before the Philadelphia County Medical Society, delivered and published in February, 1863. Although Dr. Stillé designates the disorder described by him as 'palpitation' of the heart, his account otherwise agrees so well with my own observations, that I infer a general identity of the cases studied by us.

"To be distinct in my description, it will be necessary first to say, that, during a seven months' term of service in an army hospital, attending in a ward averaging about eighty patients, I have met with the usual variety of cardiac affections. Acute endocarditis and pericarditis were the most rare. Of valvular disease a few instances presented themselves; but knowing the lax manner in which, at the beginning of the war, the examination of recruits was conducted, it was impossible to judge satisfactorily of their antecedents or origin. Dilatation of the heart, with or without the signs of thickening of the walls, occurred in quite a number of cases; especially dilatation without evidence of true muscular hypertrophy. But, although a portion at least of these cases ranged themselves under the same probable causation as those to which I wish to call particular attention, yet the large *majority* of examples of heart-disorder, under my observation, were not cases of either form of enlargement of the heart. Exclusive, then, of a very few instances of valvular disease; of a few more of pericarditis with effusion; and several of dilatation with hypertrophy, and of dilatation with attenuation; there yet remained two other classes of heart derangement in our army wards. One, not rare anywhere, was *palpitation*, or functional disturbance of the heart's action, from sympathy with irritated stomach, from nervousness, abuse of tobacco, &c.; a symptom frequently connected in our cases as elsewhere, with anæmia. But, lastly, the largest number of all must be separated from all of these, and may be properly designated, so far as I can understand them, as cases of *cardiac muscular exhaustion*.

"The symptoms of this were, rapidity with comparative feebleness of the pulse while the patient was at rest; great acceleration of the heart's movement on the *slightest* exertion; an impulse which, in proportion to its acceleration, was rather below than above the normal average of force, and was sudden and short, not heaving; dyspnoea or 'shortness of breath' after moderate exertion, especially if continued. The general condition of the body accompanying these symptoms was not uniform. Mostly anæmic at first, or cachæmic, as were nearly all the men sent to our hospitals from the army of the Potomac during the latter half of last year—the cure of the anæmia was not nearly always the cure of the heart affection.

There remained with us, after several months, a number of men, some of whom had the *aspect* of average health; with sufficient flesh, fair colour, and even tolerable muscular strength; but with a pulse of 85 to 95 when at rest, and running up to 120 or 130 upon walking slowly a few yards; and overcome altogether by standing for a few hours with muskets as hospital guards, or acting even as nurses or messengers. The physical signs observed in these cases were as follows: No extension of dulness of resonance on percussion (I mean now in the *majority* of these cases; having already mentioned that several instances of dilatation occurred in the same wards) beyond the usual limits, and sometimes being even less than natural; impulse, as already stated, without unusual force, and especially deficient *relatively* to its acceleration; having also, a short, although hardly a jerking movement, but quite different from the *heaving* movement of concentric hypertrophy, and not lifting the ear or stethoscope so much even as in many cases of transient functional palpitation. Sounds of the heart, *free from murmur* in almost all the cases: in all that large number in which no complication could be supposed to exist with the cardiac exhaustion. Anæmic murmurs, even, were quite rare. There was present, however, a comparative deficiency in duration and loudness of the first sound, and an approximation of it in character to the second sound; similar to that which is described by Stokes (although the same sign had been observed by Laennec, and was still more clearly defined by Louis) as occurring in softening of the heart in typhus or typhoid fever. In instances in which great debility, from intermittent disease, existed, the sounds were both quite deficient in strength, while the impulse was very feeble, and the rapidity of the heart's action was extreme. The last case in which my attention was called to these signs was that of R. K.—, a member of the Anderson Troop, of this city, who, after long and severe hardship and exposure, was attacked with typhoid fever followed by acute phthisis, and died under my care in May, 1863. The pulse of this patient, for *more than a month* before his decease, would have been pronounced by any physician to be that of a *moribund* person; of one who could not live forty-eight hours, and the heart-sounds and impulse were correspondingly rapid, short, and weak. Having, from previous attendance, some years since, known his ordinary pulse, which was not peculiar, I concluded that this state of his circulation must be owing to a condition of the heart identical with what I have described as occurring in our hospital patients, and which became familiarly known among us at the hospital as 'trotting heart.'

"Autopsic examination was not available to any extent in the investigation of these cases; since the affection, in the hospital at least, was not mortal. In inspecting the bodies of some patients, who died from other causes, whose circulatory apparatus had presented the symptoms above described, we found the heart attenuated, flabby, and pale. No minute examination of it was made in either of these, our attention being called to the organs more especially involved in them. While thus without direct evidence of fatty or

other degeneration of the heart in the typical cases considered, I entertain no doubt of the affection being essentially of an *atrophic* character, of which such degeneration would be a natural sequence or attendant, if circumstances did not favour restoration to health.

"Allusion has been made already to the analogy suggested by the description by Dr. Stokes, of softening of the heart in typhus fever. The most important difference between this and soldiers' heart-exhaustion is probably connected directly with the dissimilarity in causation, in all respects except in the induction of atrophy in both. In typhus or typhoid fever, the morbid state of the blood—the *pyrosis*—alters the nutrition of the heart as well as its action. In the soldier, the heart is injuriously affected by long-continued over-exertion, with deficiency of rest, and, often, of nourishment.

"To explain this more fully we must recall the circumstances of the peninsular campaign of the army of the Potomac, from which most of our cases were brought; holding in view, at the same time, a very familiar physiological principle in regard to nutrition. This principle is, that while a muscle, or other organ, will grow stronger and larger with increased exercise, so long as sufficient intervals of *repose* are allowed, and sufficient *nourishment* and other healthy conditions of repair are obtained; the contrary effect, or exhaustion and atrophy, will follow an increase or excess of exertion, without sufficient repose, food, or other healthy conditions. Now, in the campaign of McClellan on the peninsula, the soldiers suffered from great and prolonged over-exertion with the most unfavourable conditions possible—privation of rest, deficient food, bad water, and malaria. The heart, being called upon to supply the demands of the over-tasked body, must in such a case, become weakened, and that weakness is slow in being recovered from.

"The only other supposable hypotheses in regard to the affection under consideration are:—

- "1. That it is merely a symptom of general *anaemia*.
- "2. That it is a *scorbutic* symptom; and
- "3. That it is a variety of *palpitation*, dependent on usual causes, such as excessive use of alcohol, tobacco, or coffee, or self-abuse.

"As to anaemia, it has already been said, that in a number of cases, the heart-affection lasted long after the patients ceased to be anaemic, when they had enjoyed good appetite and digestion for two or three months, and had gained flesh and colour.

"Scorbutus undoubtedly was a prominent element in the pathological state of very many of the patients from the campaign of the peninsula. Its recognition was of the highest importance; but, as to its relation to the 'trotting heart' of our soldiers, setting aside the indefiniteness yet belonging to the use of the term 'scurvy' as an entity, it may suffice to remark, that, as in the case of anaemia, the scorbutus was cured, but the heart-disorder remained long afterwards.

"Nor can I think of assenting to the inclusion of our cases under the term of palpitation of the heart. The cardiac movement was different in character (as already described) from ordinary sympa-

thetic or nervous palpitation ; less heaving in impulse, more constant in character, and much more susceptible of increase by the slightest exertion. Of the ordinary causes of palpitation, some were present, of course, among the soldiers. Excessive use of whisky, of tobacco, and possibly even of coffee, was not absent altogether, even in the hospital ; masturbation, in some cases, was not impossible. But, bearing all these in mind during the almost daily study of these cases for months, with all the opportunities for vigilance furnished by the régime of an army hospital, my conviction was very positive that none of these causes could have more than a partial or secondary influence in producing the condition of the heart described. I believe it to be most correctly designated as cardiac muscular exhaustion and atrophy.

"In regard to the *prognosis* of this affection, my experience has been sufficiently extended only to justify the expectation that recovery would be slow, but might be hoped for under the most favourable circumstances, in young patients of previously good constitution. Several months of rest and treatment in the hospital failed to do more than *improve*, without nearly curing, a large proportion of our well-marked cases."

ART. 52.—On the Physiological Effects of the Derivation of Blood.

By DR. THEODORE JUNOD.

(*Lancet*, October 22, 1864.)

The object of this paper is to describe the effects of a vacuum apparatus applied to one of the limbs of the human body ; the condition of the blood in the limb so treated being expressed by the terms "haemospasis" and "hyper-haemospasis."

A student of medicine being willing to submit himself to experiments with a view of ascertaining the physiological effects on the body in a state of health (1) of haemospasis, and (2) of hyper-haemospasis, the following experiment was made :—

At eight A.M., before breakfasting, Mr. X. was placed on an inclined plane in a position nearly vertical. The pulse was then 75 in the minute ; the respiration natural. A thermometer applied to the temple stood 95°, to the hand at 86°, to the axilla at 98°, and to the left leg at 92° F. The right leg was then brought into the condition of haemospasis by means of a glass exhauster, or, in other words, submitted to the vacuum apparatus. Presently it was seen to become red and to increase in volume, and a sensation of heat was also experienced. The superficial veins gradually disappeared, in consequence of the increased fulness of the cutaneous capillaries. At forty minutes past eight the pulse was found diminished in volume and a little increased in frequency, and the face began to grow pale. A slight sensation of coldness was felt at the eyelids, ears, and hands.

Such are the principal phenomena which the application of the vacuum apparatus produces.

Mr. X. being willing to submit to a more powerful effect, the apparatus was then applied; and it may not be uninteresting to describe the effects produced, although in practice it is seldom necessary to produce the condition spoken of as hyperhaemospasis, except in some special cases.

At twenty minutes past nine—that is, in one hour and twenty minutes—the voice became more and more feeble; the pulse became thready, and its volume did not exceed that of an infant, the rate of pulsation being 90 per minute. The thermometer marked at the temple 86° , at the hand 84° , at the axilla 97° , and at the left leg 85° F. The inspirations were less frequent and deeper; the chest was more resonant on percussion; and the patient began to yawn, the yawning becoming gradually more frequent. At thirty-five minutes past nine the pulse suddenly fell to 40 pulsations, and a gentle heat made itself perceptible at the epigastrium and rapidly extended upwards to the forehead, which became bathed with perspiration.

This general relaxation is the first phase of a passing weakness, the precursive sign of lipothymia or fainting: the pupil is dilated and the power of vision lost; the senses of smell and taste nearly disappear; and "tinnitus aurium" obscures the faculty of hearing, which of all the senses is that which lasts the longest.

Although this young man could no longer see the window before which he was placed, he nevertheless described perfectly all his sensations, and his intellectual faculties remained unimpaired. We can understand that at this degree of haemospasic anaemia it may be possible to produce at will complete anaesthesia when it can be produced with the same advantage by the use of chloroform.

At forty minutes past nine the experiment was concluded, the inclined plane was reversed, and all the senses instantly recovered their power. The power of walking was not sensibly impaired, although the leg remained enlarged as much as two inches and a half in its circumference. The skin was studded with small red points, and the temperature was increased about seven degrees Fahr. All the powers were entirely restored, and the appetite was increased.

On the following day, after a calm and prolonged sleep, the extremity in which haemospasis had been produced still continued enlarged to the extent of one inch in circumference. On the third day every trace of "derivation" had disappeared.

Dr. Junod has found it difficult to produce even the appearance of syncope by these means in young persons, and the more so in proportion to their youth. This has been especially the case when any cerebral excitement or fever existed. May this peculiarity of children be accounted for by the cerebral vessels being larger in proportion in them than in the adult?

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 53.—*On the Use of Alkaline Lactates in Functional Disturbances of the Digestive Organs.*

By M. PÉTREQUIN, of Lyons.

(Journ. de Méd. et Chir., Prat., Septembre, 1864.)

The author does not address himself to the management of inflammatory or organic disease, but of functional disturbances, such as gastralgia, gastrodynia, pyrosis, acidity of stomach, loss of appetite, indigestion, flatulency, and the manifold varieties of dyspepsia. Taking into account the important part performed in the various stages of digestion by the alkaline lactates and by free lactic acid, M. Pétrequin prescribes the lactates of soda and magnesia, which are more especially present in the system, and to the accidental absence or insufficiency of which he deems that the greater number of the above-named disturbances may be ascribed.

In his first memoir, presented two years since to the Academy of Medicine, the author stated that he had been in the habit of exhibiting lozenges and powders in which the lactates of soda and magnesia were combined with sugar. Desirous of dispensing with the sugar, and administering a more fixed compound possessed of the properties of both the salts, M. Pétrequin requested an expert chemist, M. Burin Dubuisson, to endeavour to prepare a double lactate of soda and magnesia. This gentleman has succeeded in effecting the desired combination, and in obtaining a highly effective preparation well calculated for all medicinal purposes, and which is administered in eight-grain powders and in lozenges, each of which contains one grain of the drug.

M. Pétrequin separately considers the digestive process in the mouth, in the stomach, and in the intestines—a division which will be found convenient to assist the diagnosis and facilitate the treatment of the various disturbances of the function. It has the further advantage of imparting a greater degree of lucidity to the author's explanation of the rationale of the mode of treatment he has for some years advocated, and of which further experience has confirmed the value.

The saliva may be modified in quantity or in quality, hence a twofold series of morbid phenomena peculiar to the first stage of the digestive process.

The saliva is alkaline in health, but in certain morbid conditions may become acid, a phenomenon mainly attributable to irritation of the *prima via*, which it also has a tendency to keep up. This change in the saliva is removed when the gastro-intestinal disease is subdued, and the digestive functions then resume their regularity. To effect this purpose, M. Pétrequin recommends two or three lozenges of lactate of soda and magnesia to be allowed to melt in the mouth, both before and after meals. This treatment usually succeeds in correcting the acidity.

The saliva may also be deficient in quantity, and that condition is established which M. Pétrequin terms *dyspepsia from insufficient saliva*. The amount of the secretion in four-and-twenty hours in man exceeds two pounds, a quantity which appears to be necessary for healthy digestion. When, therefore, the supply fails, indigestion follows, and the proper amount of fluid may be restored by the administration of two or three lozenges of the double lactate before and after the repasts.

The indications are of a more complicated kind in the *gastric* period of digestion, on account of the increasing number of functional disturbances. Acid dyspepsia, for instance, may consist merely in acid eructations, or in heartburn, followed by vomiting. In either case M. Pétrequin prescribes 4-8 grains of the lactate before eating, and two or three lozenges afterwards.

The treatment is the same for that kind of dyspepsia to which diabetic subjects are liable.

In *incipient indigestion*, referable rather to a peculiar and morbid condition of the system than to excess, M. Pétrequin promotes the activity of the secretion by the exhibition of one of the above powders, or of a few lozenges.

The alkaline lactates are likewise beneficial in flatulency, gastralgia, &c., either taken immediately after meals or a short time later, when the symptoms of discomfort first make their appearance.

In that form of dyspepsia which the author calls *neutral*, the digestive functions are impeded by the probable insufficiency or alteration of the gastric juices. Now, alkaline substances increasing the activity of these secretions, and acids, more especially lactic acid, promoting the chymification of animal food, the alkaline lactates must prove particularly serviceable.

When the gastric juice is altered in its composition, the alkaline lactates are clearly indicated. "The gastric juice," says M. Dumas, "contains two agents—viz., *an acid*, which distends and softens azotised substances; and *pepsine*, which determines its liquefaction by an action analogous to that which diastasis exercises on starch." M. Corvisart has suggested the propriety of exhibiting pepsine when the gastric juices are deficient; and this method, based upon scientific induction, has certainly been useful. But the treatment is in some measure incomplete, because the gastric secretion contains an acid in addition to the ferment. It is, moreover, a fully demonstrated fact that digestion requires the combined operation of an acid and of pepsine, and that, of all acids, the lactic is the most efficient. The author concludes from these remarks that, in order to imitate as closely as possible the natural process, pepsine should be acidulated by the addition of lactic acid, or of the alkaline lactates. He, therefore, instructed M. Burin to prepare lozenges containing two grains of pepsine and one grain of lactate of soda and magnesia. As a substitute, he sometimes prescribes a powder containing four grains of each substance, one half of which is taken before and the other half after each meal.

Dr. Foltz, Professor of Physiology at the School of Medicine at Lyons, has frequently prescribed this medicine, and does not hesi-

tate to declare that it is one of the most efficacious preparations in the Pharmacopœia.

The process of *intestinal* digestion is more obscure than those above alluded to. We are, however, acquainted with the fact that the digestion of animal food, of farinaceous substances, and of fat, is here perfected. The intestines, like the stomach, are liable to *dyspepsia*; *borborygmi* and *meteorism* correspond with flatulent *dyspepsia*, *enteralgia* and *nervous colic* with *gastrodynia*, *relaxation* with indigestion, and certain forms of *diarrhœa* with acid *dyspepsia*. Analogy guides us also in the management of these functional disturbances, and the alkaline lactates again prove useful auxiliaries to diet, both by their influence on the salivary and gastric fluids, and on the intestinal secretions necessary to the accomplishment of the final process of digestion.

M. Pétrequin concludes by the description of a method which he has found efficacious to restore lost appetite in certain varieties of *dyspepsia*. He recommends light food, taken at very distant intervals, during which the patient is instructed to allow gum to melt in his mouth, in order to prepare the stomach to healthy secretion. A short time before meals, one or two lozenges of lactate of *soda* and *magnesia* are exhibited. The desire for food promptly returns, together with the powers of digestion. "This artificial appetite," says the author, "often effects a complete cure."

ART. 54.—*On the Pathology and Treatment of Aphthæ.*

By Dr. WORMS.

(*Gaz. Hebdom. de Méd. et Chir.*; and *Journ. de Méd. et Chir. Prat.*, Juin, 1864.)

From minute examination of the deposit on the surface of aphthæ, Dr. Worms concludes that this disease consists of a fatty matter, which is not to be found in any other disease of the mouth, and which exclusively characterizes aphthæ. The epithelium rises and soon breaks, exposing to view a yellowish secretion, previously discernible through the transparent cuticle, and of which the microscope and chemical tests invariably show the sebaceous nature. It may, on the other hand, be remarked that aphthæ are never met with on the anterior portions of the mucous membrane, where anatomists have failed in discovering any muciparous glands and where *herpes labialis* more commonly occurs; hence Billard's surmise that aphthæ are a disease of the mucous follicles, characterized by a peculiar sebaceous deposit, acquires additional probability.

In Dr. Worms' opinion aphthæ are, therefore, but the *acne* of mucous membranes.

This practitioner infers from the solubility of the exudation in ether, that this substance may be a useful local remedy for aphthæ. The pain caused by the eruption, and the difficulty of checking its progress, are well-known features of the disease, and the inefficacy of cauterization, chlorate of potash, anodynes, and other methods of treatment usually prescribed, is very generally acknowledged. Dr.

Worms has, on the contrary, resorted to ether with much benefit; this remedial agent removes the yellowish secretion, a new epithelium promptly forms, and no trace of the superficial ulcer remains beyond slightly increased vascularity of the mucous membrane. Ether may, therefore, be applied locally with advantage, but the fact of the frequent connexion of aphthæ with gastric disturbance must at the same time be borne in mind.

ART. 55.—*On Insufflation as a Remedy in Intussusception.*

By DAVID GREIG, of Dundee.

(*Edinburgh Medical Journal*, October, 1864.)

Dr. Greig relates five cases, occurring in his own practice, in which this mode of procedure was adopted. Four of these cases terminated satisfactorily: one proved fatal. The first two cases are these:—

CASE 1.—M. S. G.—, a stout, healthy, female child, six months old, always enjoyed good health, never having had a day's sickness; never had any food except breast-milk; never troubled with diarrhoea or bowel complaint. Was in her usual good health on Monday, 13th October, 1862, up to six o'clock in the evening, when, without any obvious cause, she suddenly became fretful, kicking with her feet, bending the body backwards, and screaming. In about ten minutes she became very sick and vomited severely. The skin became cold and clammy, the countenance pale, and the lips livid. In a little while she revived, but soon became restless and as sick as before. She seemed to have great pain in the abdomen, which came on in paroxysms, and to increase in intensity until she vomited, when she would seem relieved a little, or at least so faint and sick as not to scream. When given the breast, it would take it readily; but as the sickness and vomiting, with a paroxysm of pain, immediately came on, she latterly refused it. Immediately when she was seized a spoonful of castor oil was given, and hot fomentations were applied to the abdomen. The castor oil was soon ejected from the stomach, as was also a small purgative powder which was given. A warm-water enema was attempted to be administered, but the bowel seemed to be in such a state of spasm that none could be thrown up. About 8 P.M., tenesmus came on, and she passed a little fluid blood, which continued to come with every paroxysm of pain during the night. The abdomen was soft, slightly tympanitic, and not painful on pressure, except during a paroxysm of pain, when pressure seemed to increase it. On deep pressure being made over the abdomen, under the umbilicus, a very distinct hard tumour was felt, a little to the right of the mesial line. Seeing that the case was a serious one, and as the little patient was a near relation of my own, I asked my friend Dr. Pirie to take charge of the case. He attempted to give another enema, but with no better success, owing to the very peculiar spasmodic state in which the bowel was. On the morning of the 14th, as the child was no better, and as Dr. Pirie suggested an intussusception, he requested Dr. Nimmo to see the case along with him. In consultation it was decided that it was a case of intussusception of the bowel. The sickness still continued, but not so severe as on the previous day. The infant took the breast readily, and after taking it would lie still for a few minutes; pain would then seem to come on with sickness, and the milk would be ejected from the stomach with great violence, after which the child would seem exhausted and lie still for some time. It seemed to have great thirst, and took cold water greedily, which, however, was soon

ejected. The pulse was about 130, small. Injections were again administered, but with the same result as formerly. As everything had been tried, and nothing had done any good, and as it was evident the child was fast sinking, it was proposed to use the air injection which is mentioned in Dr. West's work on "Diseases of Children," as having been used with success by Mr. Gorham. Fortunately, Dr. Nimmo had in his library the volume of Guy's Hospital Reports which contained Mr. Gorham's paper on the subject, and after each of us had perused that paper, it was resolved to give the remedy a trial, as a last resource, and in truth with no very great hopes of benefit.

The nozzle of a small pair of bellows was introduced into the anus, and air injected to a considerable extent. Contrary to our expectation the air passed readily into the bowel, and seemed to give the child great relief. After the injection it lay very quiet, as if asleep, and evidently quite free from pain. In about twenty minutes from the time the air injection was administered, a slight rumbling noise was heard in the child's abdomen, followed by a crack so loud and distinct as to alarm the attendants in the room, who thought something had burst in the child's bowels. The child, however, continued as if asleep and free from pain, and in about half an hour a large feculent fluid stool, slightly mixed with blood and mucus, was passed without pain. During the night the child rested pretty well, had no return of vomiting, took the breast as usual, and in two days was quite well.

CASE 2.—W. C.—, a fine, healthy, male child, nine months old, never having had a day's sickness, was taken ill on Friday, the 7th August, 1863. He was quite well in the morning, and his bowels were freely moved about 5 A.M. At 12 noon, without any warning, he was suddenly seized with very severe sickness and vomiting ; he became deadly pale, and his skin was covered with a cold clammy sweat. In a short time he recovered from this state ; he became warm, and his countenance resumed its natural appearance. He was, however, restless and seemed to have thirst, but immediately on getting a drink of milk he fell into the same faint state, and the milk was ejected from the stomach with violence, the child before the vomiting evidently suffering great pain. Even a spoonful of cold water was not retained above a few minutes. The child continued in much the same state until I saw it at 11.30 P.M. At that time it seemed to be suffering from paroxysms of pain, which seemed to come on about every twenty minutes, and the sickness and vomiting continuing almost as bad as at first. The skin was warm and moist, the countenance pale, pulse 120, thirst very great, and everything liquid was taken with great eagerness ; it even took the breast with as great relish as it did when in health. Immediately after anything was taken into the stomach, it would lie quiet for a few minutes ; it would then become restless, and this restlessness would increase until, during one of the paroxysms of pain, everything would be ejected from the stomach ; and this was repeated again and again. The abdomen was flaccid and soft ; but a distinct, hard swelling, or tumour, could be felt under, and to the right of the umbilicus, which seemed to be somewhat tender on pressure. A spoonful of castor oil had been given in the afternoon, but was immediately vomited. The bowels had not been moved, but there was some tenesmus, and before I had been long beside the patient, about a spoonful of slime and florid blood passed from the bowel. I had not the slightest doubt the case was one of intussusception, and proceeded to administer a warm-water enema ; but the same peculiar spasmoid state of the bowel, which was mentioned as existing in the former case, was again found here. In order to overcome this I tried to plug the injection-pipe into the anus with lint ; but it was of no use, the injection seemed to give the child great pain ;

very little of the water would go up the rectum, and the little that I did succeed in throwing up, remained a very short time, and only brought away more blood. Nothing came from the bowels but fluid blood and frothy mucus, in which were found shreds of mucous membrane. This state of matters continued until 2 o'clock on Saturday afternoon, when I resolved to try the insufflation of the bowel.

Having got the tube of an ordinary elastic enema fitted on to the pipe of a small pair of bellows, I threw a considerable quantity of air into the rectum. It passed very readily into the bowel, there was no straining or attempt to expel it, and it seemed to give the child great relief. It had taken some milk from the breast shortly before this, and although the sickness was coming on as usual with a paroxysm of pain, the insufflation seemed to relieve the pain, and the milk was not vomited. I waited for an hour, and as the sickness was again threatening to come on, and fearing I had not thrown in enough air, I administered another insufflation, and continued the process until the belly showed signs of considerable distention, and the child seemed to be somewhat uneasy in consequence. At this time I also administered a teaspoonful of castor oil, and was pleased to see the stomach retain that also; and from this time there was no more vomiting. After this the child fell asleep, and slept for about half an hour, awoke, was inclined to be amused, but still seemed uneasy. Warm fomentations were applied to the belly, which seemed to give relief. From this time the child seemed free from pain, and for the most part slept until about five o'clock, when a copious stool was passed, fluid in consistence, and mixed with some blood. From this time the child was quite well, took the breast with eagerness, retained what it took, bowels became regular, and it soon made up in flesh what it had lost during its short but severe illness.

ART. 56.—*Case of Ileus treated by Insufflation of Air, &c.*

By Mr. W. H. MORGAN, Assistant-Surgeon 23rd Regt.
of Light Infantry.

(*Madras Quarterly Journal of Medical Science, Jan. 1864.*)

CASE.—20th July, 1863.—Jaffer Khan, a private in the 23rd Regiment, was brought into hospital at four P.M., complaining of severe pain in the abdomen, which came on about eleven o'clock in the forenoon. Patient states that his bowels were moved at noon, and that the stool was scanty and contained some blood.

R Ol. Ricini 5ss. immediately.

To have turpentine stupes to the abdomen should the pain continue.

Six P.M.—Bowels not moved since the oil was taken.

To have an injection of

Ol. Ricini, 5ii.
Aquaæ fervent: Oj.

This brought away some *scybala*.

21st.—Passed a restless night. He has an anxious expression of countenance, and rolls about in bed as if in pain. Abdomen tympanitic, recti muscles tense, the patient can bear pressure on the former. There is no external manifestation of a hernia. Hypogastrium dull on percussion: the man passed a quantity of urine during the visit. Pulse 96, small and weak; extremities cold; vomited three round worms last night.

Continue the stupes three or four times a day.

To have a warm hip-bath, and while in it an injection of castor oil.

Vespere.—Vomited all the water he drank since this morning, but retained a draught of chloric ether and ammonia, which he took at noon. At half-past twelve o'clock he took a pill of

Calomel gr. x.

Opium gr. i.

At three P.M. the skin was warm; pulse 144; and the patient quieter. Several galvanic currents were passed through the abdomen, and a small bleeding (Siv.) tried without any effect on the bowels.

B Calomel. gr. ii.

Opii. gr. $\frac{1}{4}$. Ft. pil.; one every third hour.

22nd.—Vomited about Oij. of stercoreaceous matter at five last evening, and another round worm. Patient slept well last night, the vomiting having greatly relieved him. Abdomen still tympanitic, pressure can be borne over every part of it; pulse 96.

Continue pills of calomel and opium every four hours.

To have a large injection of castor-oil and warm water at noon.

Vespere.—Vomited a quantity of greenish stuff shortly after taking the injection. The latter seemed to pass as far as the *cæcum* in the right iliac region and no further. It brought away some more scybala.

From this date to the 26th there was no stercoreaceous vomiting; the calomel and opium pills were continued, and also injections of castor-oil and warm water. The latter usually brought away very small quantities of feculent matter. On the morning of the 26th I detected some fragments of a tobacco-leaf in the pan, and understood from the patient that he had previously introduced them into the rectum with his fingers.

27th.—Vomited Oiv. of stercoreaceous matter yesterday; pulse 96.

Continue pills and enemata.

28th.—The stercoreaceous vomiting continues; slept better than usual. Patient was put under chloroform, and four pints of tepid water injected into the bowels.

B Calomel. gr. iij.

Ext. Colocynth. co. gr. xij.

Ipecac. gr. iij.

Ext. Hyoscyam. gr. iii. Ft. pil. vj.

One to be taken every four hours.

These pills were discontinued on the following day, and calomel and opium continued as before, with enemata. There was no change in the symptoms, and I despaired of doing anything more for the patient. He was seen on the morning of the 31st by Drs. Reynolds and Kennedy, and as a last resource it was resolved to inflate the bowels with air. At three P.M. on that day by means of bellows from the "suspended animation apparatus," and with the assistance of Assistant-Apothecary Callaghan, a quantity of air was pumped in *per anum*, until coils of intestine could be distinctly felt by passing our hands over the abdomen. The patient, who was questioned during the operation, said he felt relieved, but to us he appeared in *articulo mortis*, and I left the hospital expecting to hear of the man's death before my next visit. Great was my surprise, however, at learning on the following morning, that since ten P.M. he had had four copious feculent motions. Since this he continued to improve, and in a fortnight he was discharged. I ought to state that the gums were tender on the 1st of August, the day after the bowels were relieved.

ART. 57.—*A Case of Ileus, in which the Direct Application of Galvanism to the Intestine was used successfully.*

By Dr. J. M. FINNY.

(*Dublin Quarterly Journal of Medical Science*, November, 1864.)

Dr. Finny thinks this to be the first case in which this mode of treatment was adopted, but in this he is mistaken. The case itself, however, is not the less deserving of attention on this account.

CASE.—Robert Fox, labourer, was admitted, from Howth, into the Meath Hospital, under Dr. Stokes, on the 29th of June, 1862. The symptoms described are those of a severe case of ileus, accompanied by fecal vomiting. The treatment pursued at first was the conventional treatment, in which opium and calomel pills figured most conspicuously. The patient had been ill for two days before admission into the hospital, and for the week after admission he continued to lose ground. We quote Dr. Finny's report from the case when the galvanism was resorted to.

July 6th.—Bowels have not been relieved by the pills. The patient spent a very miserable night, as copious faecal vomiting has continued, preceded by painful hiccup.

The patient is very restless, throwing his arms about, and frequently changing his position. He is covered by a clammy sweat; his face is drawn, and expressive of great anxiety; eyes sunken and heavy.

As all the treatment hitherto adopted has entirely failed in producing any decided and beneficial effect in evacuating the intestines, and as faecal matters have been ejected by the mouth almost continuously for more than three days, ever since the night of the 2nd, with the intermission of about fifteen hours on the 4th; it is evident in such a case that unless some more speedy and efficacious treatment be employed, the patient cannot be expected to exist much longer.

Under these most serious circumstances the application of the galvanic stimulus to the mucous surface of the intestine, was proposed by Dr. Stokes as a *dernier ressort*. The manner in which it was applied is as follows:—

The patient was directed to place himself on his hands and knees, and while in this position, with his buttocks well raised and his head low, I passed up the long tube with the intention of administering a large enema of ordinary house medicine, previous to the application of the galvanism. The tube passed up readily as far as seven or eight inches, when it met some obstacle to its progress. On its withdrawal some feculent matter was observed at its end. It was again re-introduced, and by a steady pressure and frequent injections, it slowly passed through some solid obstacle without at all conveying the idea of elasticity. Once through, it was readily passed up the intestine to its full length, and the remainder of the injection thrown up. On removal of the syringe there was no escape of flatus or return of the injection. The exhausting syringe was now applied to the external extremity of the tube, but failed in causing any flow through it. Without withdrawing the tube the patient was placed on his side, and one sponge, connected with the negative pole of the electro-galvanic battery, being applied to the margin of the anus, the other sponge, in connexion with the positive pole, was applied in a rapid manner over the whole external surface of the abdominal walls. After a few such applications there was a gush of fluid matter through the tube, which was then withdrawn, and the sponge

which had hitherto been applied to the verge of the anus, was, along with a portion of metallic handle to which it was attached, passed up the rectum two or three inches, while the same proceeding as before was renewed with the other sponge. Each sudden application threw the whole abdominal muscles into the greatest spasm, and was immediately succeeded by a gush of fluid faeces. Three large basinfuls were thus got rid of. The operation, which had lasted about ten minutes, and which was attended with the most intense agony, so much so that the patient writhed under it to a painful degree, was now suspended, as great prostration of strength had ensued, the patient's pulse becoming almost imperceptible. He soon, however, rallied on the administration of brandy, and expressed himself greatly relieved. He then fell into a quiet sleep. In the afternoon he had two fluid stools, and eight more during the night entirely devoid of pain. No hard lumps were noticed to have been passed at any of these stools.

July 7th.—The patient is greatly improved in appearance; he has had an easy and painless night's rest, and feels very much better; he has had no return of the hiccup or vomiting, and complains of no pain in the abdomen even when pressed upon; his appetite has already improved, and he wishes for a chicken for dinner.

July 9th.—Since the 7th he has rapidly recovered, his bowels up to to-day having acted regularly, and of themselves. The patient is, however, weak, and occasionally listless and heavy, as if his nervous system had sustained a severe shock.

Ordered a few compound rhubarb pills; oil to be taken at night.

July 11th.—The patient left hospital to-day a little weak, but feeling otherwise perfectly well; his bowels are regular, and his stools natural.

On July 26th he again applied for admission, suffering from symptoms similar to those he presented before, but in a very less degree; his bowels having been confined for a few days, and complaining of some pain in the abdomen. He states that since he left hospital, on 11th, he has had two or three slight returns of abdominal pain, and that on one occasion his left leg swelled to a large size. These symptoms, however, left him after lying in bed for a few days. His present complaints are evidently augmented by fear, as he is very nervous about himself, and dreads a repetition of his late treatment. This, however, was not required, as, after a terebinthinate enema, a course of gentle purgatives, such as castor oil, administered every other morning, was sufficient to keep his bowels regular, and to free him of all pain.

This treatment was continued for about twelve days, and his strength and general appearance being much improved, he left hospital on 18th August. Since then he has had no return of these attacks, and has been able to resume his work as a labourer.

ART. 58.—*A Case of Xanthopsý, or Yellow Vision in Jaundice, with Observations.*

By Dr. LAW.

(*Dublin Medical Press*, May 29, 1864.)

This case, and the observations attached to it, were read at a recent meeting of the Association of the College of Physicians of Ireland.

CASE.—Michael Hutchinson, aged thirty-three, cook at an hotel, was seized a week since with pains all through his body. Soon afterwards his

stomach became so irritable that it rejected everything. He had an insatiable thirst. At the end of a week he got two pills and a draught, which gave him some relief. I now saw him, when his skin was of a pale yellow colour; his stomach was very unsettled; tongue loaded; pulse 90; has an excessive itching of the skin. All objects appear of a yellow colour. Urine of a deep orange; discharge from bowels of a pale slate colour. I directed chicken-tea for drink, and gave him a dose of castor-oil in punch. His stomach retained the castor-oil, which affected him four times. I now directed for him the following:—Pil. hydrarg. gr. iv.; pulv. rhei, gr. vi.; extract. taraxaci, sodæ siccæ, aa. gr. iii.; ol. carui, gtt. ii.; fiant pil. iv. Take one every fourth hour. Habeat haustum salin. effervescent. ter die.

From the day he took the castor oil, or from the time his bowels were affected by it, objects appeared to him more of their natural colour. His skin, however, assumed a deeper tinge of yellow, the itching of which was less; his stomach became quite settled; the appetite improved; the tongue became clean; the urine of a less dark colour, and the alvine discharge darker. At the end of ten days he was tolerably well, the discolouration of the skin being the only remaining indication of his disease. The yellow vision had entirely disappeared.

Commenting upon this case, Dr. Law says:—

"The preceding case is a very ordinary one of jaundice produced by gastro-duodenitis: the xanthopsia or yellow vision is the only feature of it that gives it a claim to being recorded. This peculiar phenomenon has been long noticed in connexion with jaundice, and the recognition of it by Lucretius may be inferred from his observation: '*Lurida præterea fiunt quæcumque tuentur arquati.*' I have a distinct recollection of two cases of xanthopsia beside the one here detailed. One patient said that the white wall of the ward in which he lay was like a sheet of brass. Although there can be no doubt of the reality of the phenomenon, as attested by so many unquestionable authorities, still it is strange how many physicians of most extensive practice have never met with it, and therefore have questioned its existence. Heberden regarded it as a medical fiction. Frank almost disclaimed its existence. Dr. Watson only once met with it. Professor Graves does not mention it, from which we may infer it had not occurred to him, as it is such a phenomenon as he would not have overlooked, and if he had seen it he would have noticed it in his clinical observations, '*de omnibus et quibusdam aliis.*' Frerichs has never seen a case of xanthopsia in jaundice, although he says he had always made inquiries for it. This certainly does appear strange, when of course he must have had considerable experience of the pathological condition to which, when it is present, it owes its existence, as is proved by his valuable clinical treatise '*On Diseases of the Liver.*' Frerichs observes that Dr. Stokes considers that xanthopsia is an indication of impending paralysis. I can say that there was no paralysis in any of the three cases of xanthopsia that came under my observation. He further remarks that Bamberger had only observed this phenomenon in jaundiced persons who had died from cirrhosis of the liver. I can with almost equal confidence assert that cirrhosis of the liver did not exist in any of the three cases that came under my observation, at least there were none of the usual signs or symptoms of cirrhosis present."

Although I have seen jaundice with cirrhosis of the liver, I believe it to be very rare, and I conceive its infrequency finds an easy explanation in the nature of that lesion, which interrupts both the secretion and nutrition of the liver. Cirrhosis produces a dingy earthy complexion, which I have often heard described as a jaundiced complexion, but which, I am convinced, has nothing to do with jaundice, as proved by the discharges from the bowels and the urine; the colour of the former indicating the presence of bile, and the colour of the latter equally indicating its absence. We have not unfrequently heard the term 'subicteric' applied to this sallow complexion when there was no reason to suppose it due to any hepatic affection, and in fact was the complexion of anaemia. We have seen cases of anaemia treated as jaundice, a mistake which would not have been made had the appearances presented by the alvine discharges and the urine been attended to. We would here strongly condemn the use of terms involving actual error as to the nature of the pathological condition to which they are applied. It is thus the term 'sub-apoplectic' has been employed to express a condition not only unlike apoplexy but quite different from it. The condition to which we refer is, in fact, syncope. This misapplication of the term has arisen out of the fact of the two morbid conditions having some phenomena in common, although they differ in most important particulars—particulars which ought to have an especial practical influence. How often has this term 'sub-apoplectic' been applied to the loss of consciousness produced by cardiac affections; and how recently has practical medicine learned to distinguish between the very different, even opposite, conditions and circumstances upon which this phenomenon depends. My sole object in detailing the preceding case is to place on record an unequivocal instance of a phenomenon which, although often spoken of, is of rare occurrence. I can say that, as far as my observation has gone, it has not been present in the worst cases of jaundice that have come in my way. Pathology has yet to determine the conditions of its existence."

ART. 59.—*On the Treatment of Ascarides.*

By M. ROGER.

(*Journ. de Méd. et Chir. Prat.*, Mai, 1864.)

The treatment should address itself to a twofold indication, viz., the expulsion of the worms, and the prevention of their re-appearance.

In the first place, M. Roger rejects all anthelmintics which act mechanically, such as zinc grains or iron filings, alluded to by Bremaer. The most reliable medicines, he thinks, are Gigartinia helminthocorton, semen-contra, wormwood, and soot for the poor, and santonine for subjects in easy circumstances.

The Corsican moss, or *Gigartinia*, may be prescribed in powder in doses of gr. xv.—xxx., in preserves or baked apples. A filtered

infusion of 3*j.*—iv. of the moss in eight ounces of milk is a most effective remedy, of not unpleasant taste.

Bremser recommends the coarse powder of *Semen-contra*, recently bruised, in ten or fifteen-grain doses, or an infusion of two or three drachms in milk or beef-tea.

Wormwood (*Artemisia absinthium*) may be given in the shape of an infusion of 3*j.*—iv. in four ounces of water or milk.

A mixture of two drachms of *soot* with an equal amount of coffee, infused in four ounces of boiling water, sweetened and strained, forms a not very tempting, but a cheap and efficient beverage.

A paste of *pumpkin seeds*, prepared with one or two ounces of seeds boiled, bruised in a mortar and mixed with honey, has also been recently recommended.

Santonine, a more expensive remedy, may be exhibited in the shape of sweetmeats or drops, flavoured with chocolate; each drop or *dragée* contains one grain of the remedial agent; one, two, three, or four grains of santonine may be given. M. Rogers seldom exceeds this quantity.

M. Cruveilhier's vermifuge syrup is always beneficial. It contains one drachm of each of the following substances, viz.: Senna leaves, rhubarb, semen-contra, southernwood, Corsican moss, tansy, and artemisia: these drugs are infused in eight oz. of water, and a sufficient quantity of sugar is added to form a syrup, a table-spoonful of which is taken for three successive mornings.

M. Cruveilhier states that this syrup has sometimes caused the expulsion of as many as sixty *lumbrixi* in one day. MM. Rilliet and Barthez prefer this preparation to Bremser's electuary, which is efficient, it is true, but so intolerably disagreeable to the taste that children often cannot be induced to take it.

Whatever vermifuge may have been selected, some aperient—calomel or castor oil, for instance—should subsequently be prescribed. It is then judicious to administer a spoonful of bark wine every evening for a week, in accordance with a precept laid down by M. Cruveilhier.

This method of treatment generally relieves the child for a time, but it is further necessary to prevent the reproduction of the worms; and strict rules of hygiene, especially as regards the water used as a beverage, must then be attended to. In this point lies the whole gist of the preventive treatment, and the attention of local administrations cannot be too earnestly invited to the important practical facts on which M. Davaine's researches have thrown unexpected light.

ART. 60.—*A Means of Diagnosis in Cases of Abdominal Tumours.*

By Dr. HENRY KENNEDY, Physician to Sir P. Dun's Hospital, &c.

(*Dublin Quart. Journ. of Medical Science*, Aug. 1864.)

The following remarks occur in a practical paper on the diagnosis of abdominal swellings, in relation, chiefly, to dilatation of the colon. Dr. Kennedy writes:—

"I wish to say a few words on a means of diagnosis which often affords valuable aid, though I am not aware that any writer has noticed it—I refer to the mobility of abdominal tumours caused by the act of inspiration. All are aware that abdominal tumours may by pressure of the hand be made quite moveable, but their movement may be in some instances caused by the act of inspiration. The first occasion on which I noticed this fact was several years back during the lifetime of the late Surgeon Neville. He requested me one morning to see a man who had a tumour in the hypogastric region, of the nature of which he was doubtful. The question was between a tumour in the stomach and aneurism of the aorta. As the patient lay on his back, I thought I observed the tumour move, and on closer scrutiny there could be no doubt of the matter, and as little doubt that it was due to the act of inspiration. It at once struck me that there was thus afforded a means by which a correct diagnosis could be made. It was obvious that if the tumour was aneurism no act of inspiration could move it, but if it were in the stomach, it was equally obvious that such might occur. Acting on this, the diagnosis was made. I pronounced it to be tumour in the stomach, and the result confirmed the accuracy of the principle, as as after the man's death malignant disease of the stomach was discovered on post-mortem examination. Since this case came under my notice, I have had many opportunities of verifying the point. It is obvious that its value applies chiefly to tumours situated in the upper third of the abdomen. A tumour not affected by inspiration must belong to the parts known to be fixed, as, for instance, the aorta. Since I observed this sign it has not fallen to my lot to meet cases of abscess in the liver; but there is little doubt that in such a case the act of inspiration would cause the tumour to move, but if adhesions existed between the tumour and any of the fixed parts, the sign would then cease—a point of no little consequence to determine, and bearing directly on the question of treatment.

"It may be observed that the degree to which movement may be given to the parts close to the diaphragm varies considerably with the interval within. Those who have long and deep chests are able to depress the diaphragm, and necessarily the neighbouring organs, much more than others. This no doubt is what we might expect; but it is curious to what an extent it exists, and how out of all proportion it is in some cases to what exists in others.

"It is not to be supposed, however, that the sign is confined to the upper third of the abdomen—it may be very well marked in the middle third. Of this I have seen numerous examples. One of these was in a boy, 15 years of age, in whom the tumour was the size of a small apple, situated on the left side between the ribs and the umbilicus, but nearer to the latter. As the boy lay on his back and made a deep inspiration, the tumour glided downwards about one inch and a half, and rose again as he expired. Several gentlemen, including Drs. Law, Hudson, Moore, Smith, and Walsh, saw the boy; but after the closest examination no diagnosis approaching to certainty was arrived at. The spleen was known to be enlarged. It was remarked that when the boy entered the hospital the veins of the abdomen

were very much enlarged, but they decreased considerably some time subsequently. The disease in this case turned out to be induration of the omentum, probably the result of strumous deposit. I had seen a similar case with Dr. Cuthbert, except that in that instance the tumour occupied the right side of the abdomen.

"In reference to the distance to which the act of inspiration may attend abdominal tumours, I saw one instance of a tumour which reached the right iliac fossa and was so moved. I could feel a distinct movement when the patient made a deep inspiration. The explanation of this would be that it was the movement of the liver above which was communicated downwards. The case was one which commenced in the liver, and grew with great rapidity until it came low down on the right side. Had it grown from below, the great probability is no effort of breathing would have influenced it. At all events, the fact that respiration is capable of affecting the moveable portions of the upper and middle thirds of the abdomen, and may often reach so far as the lower part, is a point worthy of notice here, the more so as I am addressing many who have to deal with diseases of the ovary, and this principle may be the means of detecting adhesions which I should suppose it is of great consequence to be able to make out."

ART. 61.—*On Pulsating and Aneurismal Tumours
of the Abdomen.*

By DR. HABERSHON, Assistant-Physician to Guy's Hospital.

(*Guy's Hospital Reports.* Third Series, vol. ix. 1864.)

This paper contains the detailed histories of thirteen cases of aneurismal tumour of the abdomen, and of three of non-aneurismal but pulsating tumours. These latter three were respectively of cancerous disease of the glands behind the stomach, of cancerous disease at the pyloric extremity, and of suppuration in the lesser omentum.

These clinical observations are preceded by some highly interesting and instructive remarks on the subject of abdominal pulsating tumours.

The symptoms of aneurismal disease in the abdomen are regarded in three aspects by Dr. Habershon: *the negative signs, the character of the pain, and the character of the pulsating tumour.* As to the negative signs, there is a remarkable absence of constitutional disturbance, until the patient is worn out by the intensity and long continuance of the pain. This pain is a very constant indication of aneurism; it is of a double character, one constant and uniform, the other more intense and paroxysmal; the former is wearisome and distressing by its duration, the other agonizing in its severity. The first kind of pain has been attributed by some observers to erosion of the vertebrae, but Dr. Habershon believes it to be due to the distension of nerve-filaments upon the aneurismal sac. It is

remarked by Dr. Stokes that erosion of the vertebrae may exist without pain, and pain may also exist without erosion.

The third characteristic sign of abdominal aneurism, the presence of a pulsating tumour, is thus described :—

" Aneurism generally occurs near the commencement of the abdominal aorta, in the neighbourhood of the celiac axis, and the growth extends from this part ; if the tumour enlarges upwards from the pelvis, it is more likely to be ovarian or glandular. As the tumour passes downwards, it is more frequently found on the left than on the right side ; but if the celiac axis or the mesenteric artery be affected, the sac may be found to increase directly forwards. The pulsation is generally uniform, and is not removed by withdrawing the pressure from the abdominal aorta. The uniformity of the pulsation is modified if the sac be situated behind the strong lumbar fascia, or if it contain much fibrinous deposit. The pulsation has also been shown by Dr. Lees to be diastolic in character, but it is sometimes very difficult to find an interval between the systole of the heart and the aneurismal impulse. A bruit may often be heard at the site of the tumour, but this symptom is not unfrequently absent ; and the bruit, if present, may be systolic or double ; generally, however, it is systolic. Dr. Corrigan has noticed that the murmur is at an early stage more distinct when the patient is in a recumbent than when in an erect posture, because there is less tension in the sac ; but a tumour pressing upon the aorta often communicates a systolic bruit, and one which is more distinct in a recumbent position, on account of greater pressure being then exerted upon the vessel. When regurgitation through the aortic valves also exists, we might easily be misled by the presence of an abdominal bruit, for in such cases there is much arterial throbbing, and the lining membrane of the arteries is often diseased. It is remarkable, however, that aneurism of the abdominal aorta is much less frequently associated with disease of the heart than is aneurism of the thoracic aorta ; the heart is in the former generally healthy, although we give several instances in which the reverse was the case (Nos. 8 and 9)."

" Pulsation is not always equally distinct in abdominal tumours, for when occurring at the posterior part of the aorta, close to the diaphragm, and when the sac passes *beneath the strong lumbar fascia*, pulsation is very indistinct at the earlier stages, as in No. 1. When the tumour extends forwards, impulse is more easily perceived, and it is generally felt in the left hypochondrium, or at the scrobiculus cordis. Occasionally it is first perceived in the loin, between the last rib and the crest of the ilium. As the sac enlarges, there may be visceral displacement, the liver may be pushed forward, so also the kidney, spleen, or pancreas. The thoracic viscera are also encroached upon, and dyspnoea and palpitation of the heart are induced."

Of course the diagnosis of pulsating tumours of the abdomen must be founded upon the correspondence of the symptoms presented with those described as characteristic of these tumours. In their cause, duration, and termination there is no striking peculiarity.

liarity, that is, other than as aneurismal tumours. The treatment should consist in perfect rest, the allowance of a sustaining but unstimulating diet, the regulation of the bowels by the mildest aperients, or by enemata, and, if there be arterial excitement, the administration of the mineral acids with tincture of digitalis. In the relief of the suffering, belladonna sometimes gives a little alleviation; opiates, morphia, and chloroform are alike unavailing.

This paper is accompanied by a plate, containing two figures representing the specimens derived from a case where a localized abscess in the lesser omentum formed a pulsating tumour.

ART. 62.—*An Obscure Case of Abdominal Tumour.*

By Dr. MURCHISON, Physician to the London Fever Hospital, &c.

(*Transactions of the Pathological Society of London, Vol. xiv. 1863.*)

The heading of this case is "Obstruction and Inflammation of the Femoral Vein resulting from the Pressure of a distended Sacculus of the Urinary Bladder."

CASE.—J. H.—, aged sixty-eight, was admitted into the London Fever Hospital on the 3rd of February, 1863. He was said to have been ill with "fever for five weeks, and to have been in a very destitute state for some time before. He was brought from Deptford, where typhus had been very prevalent, and his condition on admission, notwithstanding the absence of eruption (accounted for by the duration of the illness) rendered it probable that he was convalescent from an attack of typhus fever. The pulse was 84, and feeble; the tongue was dry, and brown at the base; the bowels were constive; the heart's action was very weak, but in other respects the physical signs of the chest were normal; the mind was slightly confused; there was no oedema, and the urine was free from albumen. On the left leg there was an extensive chronic ulcer, with a dirty sloughy surface. The patient was ordered a mixture containing quinine and mineral acid, together with brandy and a generous diet.

During the first few days he progressed favourably; he slept well; the appetite improved; the tongue became moist, and the surface of the ulcer cleaner. On the 13th, however, he was not so well; he had considerable epistaxis, and occasional rigors, in the morning; his cheeks were very flushed; the pulse was 96, and very feeble, and the tongue drier. The ulcer on the left leg looked clean and healthy, but for the first time the right thigh and leg were noticed to be enormously swollen—fully twice the size of the other limb. The surface was edematous, tense, slightly reddened, and somewhat tender. A hard tender cord could be felt in the course of the femoral sheath. In the lower and right side of the abdomen was a tense, very tender, fluctuating swelling, extending from Poupart's ligament for four inches upwards, and laterally from the left extremity of the crest of the pubes to the anterior superior spine of the right ilium. There was no corresponding swelling on the left side, while the left leg and the body generally were free from oedema. The abdominal swelling yielded distinct signs of fluid on percussion; its upper margin was convex, and easily defined. There was no tenderness over the spine, or in the lumbar region. The bowels had been opened on the previous day, and the patient had not

suffered from diarrhoea, nor from any abdominal symptom since admission. Neither had he complained of any pain or difficulty in micturition ; the urine was ascertained to have been passed in considerable quantity ; it was of a dark amber colour, and clear ; it threw down no deposit on standing, and was free from albumen. The right thigh and left leg were ordered to be kept elevated and fomented with a poppy-head lotion ; a poultice was applied to the abdominal swelling ; sulphuric ether was added to the former mixture ; and a diet, consisting of beef-tea, milk, and two eggs, with twelve ounces of brandy, was prescribed.

On the following day, the swelling both in the abdomen and leg were found to have slightly increased ; while the former was more tender. The patient was in a very low state, and the tongue was dry and brown. One pint of urine had been passed in the morning, and twelve ounces immediately before the visit. As death appeared inevitable, unless the swelling was relieved, a puncture was now made with a fine trocar into the abdominal swelling about midway between the pubes and the spine of the ilium. Twelve ounces of a clear fluid were drawn off. This was ascertained to be urine, and at once the discharge from the canula was stopped, and a catheter was introduced into the bladder, from which thirty-two ounces of urine were drawn off, making fifty-six ounces discharged within half-an-hour. Meanwhile the abdominal tumour had completely disappeared, and the patient experienced great relief. The catheter was tied into the bladder, but was removed during the night by the patient.

After this the patient gradually became lower, but there was no return of the abdominal swelling, and at no time any sickness, or pain, or tenderness of the abdomen. The day after the operation there was considerable restlessness, which was relieved by a few small doses of Liquor Opii Sedativus. The urine was passed regularly in considerable quantity, and was now found to be thick and muddy from the presence of mucus. The right thigh and leg were reduced in size and tension, but still remained much larger than the left. The patient gradually sank, and died at nine A.M., on the 18th February.

The body was examined at three P.M. on the day of death. No trace of the wound made by the trocar could be found internally, and there was no increased vascularity or sign of inflammation in any part of the peritoneum. On the right side of the bladder, near its fundus, was a sacculated pouch bulging towards the peritoneal cavity not larger than a walnut, but which, when distended, must have produced the abdominal tumour during life. It presented no trace of the puncture. On the left side of the bladder was another hernial protrusion, but not larger than a hazel-nut. The muscular coat of the bladder was thickened. The bladder contained a few ounces of dark turbid urine. The mucous membrane was ulcerated at many places, and of a dark hue. The orifice between the bladder and the larger sacculus was just large enough to admit the point of the finger. On close inspection, the mucous membrane presented an immense number of minute saccular protrusions through the meshes formed by the hands of the muscular fibre, in addition to the two large ones already mentioned. The prostate was but slightly enlarged, but there was considerable constriction of the urethra in front of the membranous portion.

The right femoral vein was much enlarged and distended, presenting the form of a rounded cord, whose diameter at the upper part was between nine and ten lines. This enlargement commenced abruptly at the brim of the pelvis, and extended down to the foot. The vessel also presented a nodular dilatation at each place where the main truck was joined by a large branch. On splitting open the vessel, it was found filled with dark coagulum. Along the upper four inches of the enlargement, the coagulum was firmly adherent

to the wall of the vessel, and the portion of the clot in contact with the vessel, and extending inwards for nearly a line, was firm and decolorized. Below this the clot was non-adherent, except at some of the nodular dilatations above mentioned. The right femoral artery contained a small quantity of fluid blood.

The kidneys were slightly diseased, but the other organs of the body presented no morbid appearance of importance.

Remarks.—The above case presents several features of interest. Distended sacci of the urinary bladder, as large as a man's head, have not unfrequently been observed, but that such a sacculus should form a unilateral abdominal tumour, extending as far as the spine of the ilium is an occurrence almost, if not quite, unique. That the tumour distinguished during life was due to the distended sacculus, was proved by the urine drawn off by the canula, and by the tumour disappearing when the bladder was emptied by the catheter. The contracted state of the sacculus after death was accounted for by certain bands of muscular fibre being extended over it from the muscular coat of the bladder. But the most extraordinary circumstance in the case was the obstruction and inflammation of the right femoral vein. That the condition of the vein resulted from the pressure of the distended sacculus was indicated by the following facts :—1. The abdominal swelling, and the enlargement of the right lower extremity, with tenderness and hardness along the femoral sheath, appeared suddenly and simultaneously. 2. The great tension and tenderness of the abdominal swelling showed that it was exerting pressure on the surrounding parts. 3. The obstruction of the vein commenced abruptly at the brim of the pelvis, which formed an unyielding body against which the vessel was compressed. 4. After the disappearance of the abdominal tumour, there was a considerable subsidence in the swelling of the leg. It is to be observed that the exploratory puncture made into the abdominal swelling was followed by no bad consequence. The puncture was made solely with the object of throwing light on what at the time was justly regarded as a most obscure case. The unilateral character of the swelling, the enlargement of the corresponding leg, the inflamed condition of the femoral vein, and the circumstance that the patient was passing without any difficulty abundance of clear urine free from albumen or mucus, removed all suspicion that the bladder was the organ primarily at fault.

ART. 63.—*Extensive Development of Cysticerci
in the Human Body.*

By M. DELORE.

(*Gaz. Méd. de Paris*, Oct. 3, 1863; and *Brit. Med. Journ.*, Dec. 26, 1863.)

CASE.—Pierre Massot, aged seventy-seven, was admitted into the Hôtel-Dieu, at Lyons, in November, 1862, with pulmonary catarrh and general weakness. On February 9th, 1863, he broke the neck of the left thigh-bone, and was consequently removed into the surgical wards, under M. Delore, where he gradually became weaker, and died on April 16th. M. Delore had noticed, during the man's life, a number of small tumours on the chest, along the arms, on the elbows, and in the armpits. The lower limbs were very oedematous, so that the presence of any tumours in this situation could not be ascertained. The swellings were subcutaneous, and were not adherent to the skin nor to subjacent parts. Some of these seemed to be united by fibro-cellular bands, as they were easily moved

together. The skin over them was unaltered ; they were of the size of haricot-beans, very hard, and presented no trace of fluctuation. It was thought that they were of fibro-plastic character.

Thirty hours after the man's death, the tumours were examined by MM. Delore and Bertholus, and were recognised to be due to the presence of cysticerci. Several cysticerci were found in the subcutaneous tissue of the conjunctivæ. The muscles were pale and easily torn ; all those of the trunk and limbs contained numerous cysticerci ; in the diaphragm there was one nearly as large as an almond. It was estimated that the subcutaneous conjunctival tissue and the subaponeurotic and intermuscular tissue contained about 2000 of these bodies. They occupied principally the points of insertion of the muscles ; their longest diameter lay parallel with the fibres, which they separated without destroying ; they were also lodged in the intermuscular spaces. No cysticerci were contained in the bones. The head of the thigh-bone was broken outside the capsule, and the great trochanter was also detached. Union had not taken place. There were no cysticerci in the eyes ; nor at the base of the tongue, where they are always present in measles pigs (up to the present time, only one case of cysticerci in the human tongue has been noticed ; it is related by Rudolphi). The liver, spleen, and kidneys were quite healthy ; the latter presented numerous cysts on their surface. The pancreas contained one cysticercus. The mesentery was literally crammed with them. The parotid glands contained several. Three or four were found in the sides of the larynx. There were sixteen on the surface and in the tissue of the lungs. One was placed superficially on the anterior wall of the heart. The intestines were carefully washed and examined ; but no tæniae or worms of any kind were found. In the nervous centres, 111 cysticerci were found ; viz., 22 in the membranes, 84 in the cerebrum, 4 in the cerebellum, and 1 in the medulla oblongata. None were present in the spinal cord. On the surface of the brain, a rather large number of cysticerci had formed a small cavity in the substance of the convolutions ; others were seen through a thin layer of cerebral substance. The ventricles, choroid plexus, and optic thalami, contained a considerable number. The brain was soft and diffused.

An examination of the parasites showed that the vesicles varied much in size, and that they contained scolices having a double range of hooklets varying from thirty to thirty-four in number.

Very little information could be obtained as to the antecedent history of Pierre Massot. As far as could be ascertained, he was a beggar, led a wandering life, and was frequently intoxicated. His food ordinarily consisted of bread, cheese, and pork. "In the part of the country where he lived, measles pork is common ; but no cases of tæniae have been noticed there."

Cases where the muscles and organs have been generally occupied with cysticerci are very rare. M. Delore has met with only two such instances ; one related by Werner, the other by Demarquay. In the latter case, most of the muscles contained cysticerci ; but among the internal organs, the lungs alone. In a case of ununited fracture of the humerus, under the care of Dupuytren, where resection was performed, several hydatids were found in the fragments. The fracture was attributed to their presence, as the patient had broken his arm in throwing a stone with moderate force.

(E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 64.—*On some Points in the Treatment of Diabetes.*

By Dr. ANSTIE, Assistant-Physician to the
Westminster Hospital, &c.

(*British Medical Journal*, Sept. 17, 1864.)

In a short paper read at the last Annual Meeting of the British Medical Association, Dr. Anstie says:—

"The fact has long been recognised, that great good may be effected by the regular use of a diet in which the nitrogenous matters shall be represented, for the most part, by meat, and the hydrogen and carbon by some form of oily food. It is necessary to avoid the administration of the hydrates of carbon which enter into the composition of any ordinary diet; since, in the peculiar state of the system which distinguishes this disease, unchanged sugar would be left to circulate in such large proportions in the systemic blood, that the latter fluid would infallibly exert an irritant influence on the kidney, and provoke a saccharine diuresis. Hydrogen and carbon must, therefore, be administered in the form of oil or fat rather than of starch or sugar; and it has been stated that it is not only necessary to adopt this kind of regimen, but that the stomachs of diabetic patients easily accept oleaginous food. I have to notice a class of cases in which this does not take place.

"During the last four or five years, I have met with several instances in which sufferers from diabetes not only did not readily accept, but positively loathed almost every kind of oleaginous food which could be proposed to them. This singular loathing of an aliment which is especially necessary for the patient's welfare, reminds us of the similar peculiarity which Dr. Edward Smith has noted in a large proportion of phthisical persons, and which Dr. Radcliffe has observed in the sufferers from neuralgia. It is to be met with firmness and perseverance on the part of the medical attendant, who must try one form of fatty food after another till he does find one which the patient will take. In several instances, I have tried everything in vain; till, at last, pure cream, taken in pretty large quantity, has fulfilled the desired indication. In another case, the almond biscuits proposed by Dr. Pavy answered very well.

"It may seem almost unnecessary to insist on the necessity of this particular portion of the dietary treatment; but, in truth, it is often neglected; and I am anxious to call attention to the evil consequences of such neglect. These are, of course, partly seen in the emaciation of the patient; but by far the most serious effect is the severe nervous distress which soon begins to afflict him. Nearly all these cases are distinguished by a persistent and most troublesome insomnia; and I have now attended several such patients in whom this condition had brought about the habit of opium-eating. The effect of this practice upon diabetic patients is so peculiar, that I think it worth while to relate some of the observations on this point which I have made.

"The sufferer from diabetes quickly finds out that a small dose of opium is of no good to him; the fact being that, with the abnormal flow of urine which constantly goes on, a considerable proportion of each dose is quickly eliminated from the system. Accordingly, he quickly advances to the use of a quantity of opium equivalent, perhaps, to from four or five to twenty grains daily. Having found the level of opium-consumption which gives him comfort, he does not necessarily increase the dose any further, but remains, perhaps, stationary for many months, or even years, at the same dose.

"Now, this is, doubtless, an unsatisfactory and undesirable condition for a patient to get into; but I wish to direct attention to the fact, that where once the habit has been fully formed, and the daily *quantum* of opium is not being increased, the medical attendant incurs much risk if he suddenly cut it off. Under such circumstances, both the nervous distress and the excretion of sugar are apt to increase to an alarming extent.

"On the other hand, there is no doubt in my mind that, where the patient has advanced to the use of truly narcotic doses of opium (a fact which may be known by his suffering distinct depression and languor, with great contraction of the pupils, about half an hour after taking a dose), he is putting himself in no little peril. True narcotic or paralysing action has always a tendency to aggravate diabetes; and opium, given in large doses, shares, in this respect, the properties of the anaesthetics chloroform and ether. Whenever, therefore, the patient informs us that he has carried opium-eating to an extent which involves any depressive effects, we are bound to interfere at once.

"The only true substitute for narcotic stimulants, where a diabetic patient has got to rely upon them, is the speedy adoption of a dietary calculated to improve the special nutrition of the nervous system. It is here that we particularly need to insist upon the use of a high proportion of fatty ingredients in the daily food, and (where the stomach will bear it) the medicinal employment of cod-liver oil. There is one other ingredient of nervous tissue, which is, also, not improbably indicated in these cases—I mean phosphorus. Acting on this idea, I have several times administered this remedy, with decidedly good effect. The form which I have selected is that of the hypophosphite of soda or lime, which I agree with Dr. Radcliffe in believing to be by far the most efficient preparation of phosphorus, where we desire a food-tonic to the nervous system.

"I stated that the habit of opium-eating, where the doses have not been carried to a large extent, and have remained stationary for some time, is not to be rashly interfered with; at least, till a greatly improved scale of dietary has been established. The truth appears to be that the instinct of opium-eating, dangerous as it may be, is one which has not unfrequently arrested disease at a critical moment when it was about to assume a new and more serious development. It is well known that this practice has a tendency to arrest commencing phthisis; and there is little doubt that, when not carried to excess, it has had this effect in the case of diabetic patients, who

from defective diet and other sources of depression, would almost infallibly have developed tubercle. The true moral of this observation should be an increased solicitude for that sort of improvement in the nutrition of the patient which would do away with the nervous distress, which is, after all, the central and most threatening feature in the clinical history of diabetes; and in comparison with which, the elimination of sugar and the waste of fatty tissues are but secondary and unimportant matters. If these brief observations have any effect in enforcing this important maxim, the frequent neglect of which has been exemplified by cases which have come under my notice, both in private and in hospital practice, the purpose of this paper will be answered. A considerable proportion of diabetic patients are, I imagine, practically starved; and this notwithstanding a nominally high standard of diet; simply from the need of a more plentiful supply of fat than is administered to them. Mere animal diet, or animal diet *plus* gluten bread and the less starchy vegetables, will not fill the place of this—nor will anything else."

ART. 65.—*A New Symptom in Bright's Disease.*

By Dr. FAUVEL.

(*Phil. Med. and Surg. Reporter*; and *Dub. Med. Press*, June 1, 1863.)

Dr. Fauvel calls attention to a symptom of Bright's disease, exceedingly grave and but little known. He designates it by the name of *albuminuric aphonia*. In some cases, before any visible symptom indicates the existence of albuminuria, there appears an edema of the mucous lining of the larynx. The patient feels a hoarseness, a slight trouble in the respiratory functions, the voice disappears, and suffocation may become imminent. These symptoms make their appearance in sudden manner or quite insidiously; they are in some cases intermittent, but in this case the progress of the disease always interferes with the act of respiration. Should not the attention be called to the possibility of there being Bright's disease, the accidents may be attributed to different causes, such as syphilis for example, and a wrong treatment applied for so long as the main disease is not kept in view, the different modes of treatment employed are altogether useless, if not hurtful. The employment of the laryngoscope enables the practitioner to examine the state of the larynx and follow step by step the progress of the disease. The aryteno-epiglottic folds, the mucous coat of the vestibule, the superior vocal cords, the ventriculi, and inferior vocal cords, are seen to be in a state of tumefaction.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 66.—*On the Treatment of Baldness.*

By M. HARDY, Physician to the Hôpital St. Louis, Paris.

(Journ. de Méd. et Chir. Prat., Septembre, 1864.)

Dr. Caminiti recently reported, in the *Gazette des Hôpitaux*, a lecture delivered by M. Hardy, on the different kinds of baldness and on the treatment applicable to each. M. Hardy describes three varieties of alopecia—viz., *congenital*, *idiopathic*, and *symptomatic baldness*. The first is beyond the resources of art; in the second, M. Hardy recommends the hair to be cut short, and the scalp to be strengthened with washes containing rum or brandy, or with stimulating pomades prepared with tannin or cantharides. Practically, however, *symptomatic* alopecia alone is amenable to treatment.

In chlorosis, in the anaemia consequent on post-partum affections, the practitioner's first duty is to endeavour to restore the tone of the system by the exhibition of bark, iron, and nutritious diet, after which local applications may be used with effect to remedy the debility of the capillary follicles. Alcoholic lotions, and stimulating pomades prepared with castor or croton-oil and gallic acid may then be prescribed with advantage. M. Hardy frequently recommends the following:—

M.
B. Medullæ bovinæ ʒ.i.;
Ol. ricini, ʒss.;
Acidi gallici, gr. xx.;
Tinet. rosmarinii, ʒss.

When the baldness is the result of disease of the scalp, stimulating applications are injurious, and the treatment must be instituted solely with a view to the cure of the cutaneous affection, after the disappearance of which the hair will speedily grow again.

The same remark applies to alopecia caused by the presence of parasites; the insects must in the first place be destroyed, before any attempt is made to promote the growth of the hair. When permanent baldness becomes a cause of disease, a substitute must be used for the natural ornament of the head, in the shape of a wig or cap, in order to avert the colds, neuralgic pains, and other unpleasant consequences of the unprotected condition of the scalp.

ART. 67.—*On Occipital and Constitutional Impetigo of the Scalp.*

By Dr. BALMANNO SQUIRE.

(Medical Times and Gazette, Aug. 20, 1864.)

In a paper read at the last Annual Meeting of the British Medical Association, Dr. Squire said that he had called his communication a paper on occipital and constitutional impetigo of the scalp,

because from some researches he had been making during the past year he had arrived at the conclusion that impetigo of the occipital third of the scalp is scarcely ever of constitutional origin, and that constitutional impetigo of the scalp affects usually the anterior two-thirds of the scalp, leaving the occiput free—the occipital variety of impetigo being due to the irritation caused by the presence of the pediculus capitis.

In the great majority of the cases of impetigo of the scalp (a disease which may be said to be peculiar to childhood) the eruption is almost entirely confined either to the forepart or to the hinder part of the scalp; in a minority of cases the disease is more diffused over the whole scalp, but is always worst either at the front or at the back of the head. In every case that the author had yet examined, without a single exception, he had found that whenever the disease was most severe at the occiput it was associated with the presence of pediculi, and that whenever the eruption was most marked at the sinciput no trace of the pediculus even on most careful search could be discovered.

He had also noticed that occipital impetigo differed from constitutional impetigo in other particulars besides its situation. These particulars he entered into in detail.

The question might arise, could the pediculus give rise to sufficient irritation to cause so severe an affection of the scalp? This objection the author anticipated by referring to the almost maddening irritation that was frequently complained of by persons affected by the pediculus pubis. Again, it might be asked does the presence of the pediculus capitis always produce occipital impetigo? To this the author would reply, that he had frequently seen instances where it had not done so, and he classed them with those cases, not at all rare, of affection from the pediculus pubis where the patient complained of no irritation whatever.

ART. 68.—*Practical Remarks on Prurigo.*

By M. DUCHESNE-DUPARC.

(*Journ. de Méd. et Chir. Prat.*, Mai, 1864.)

For practical purposes it is unnecessary to describe more than three forms of the disease—viz., recent and chronic prurigo, and that variety which Alibert called *latent*, because the papulae are invisible. We pass by the description of the symptoms, and confine our remarks to the treatment of the affection.

Antiphlogistic measures are those best adapted for the cure of the recent eruption. In young and plethoric subjects, when an extensive surface of the skin is effected, M. Duchesne-Duparc resorts to venesection or the application of leeches to the margin of the anus, and prescribes milk and vegetable diet, cooling drinks, such as lemonades prepared with nitric or sulphuric acid. He recommends starch or bran-baths, and frequent lotions over the skin with decoctions of lettuce, solanum nigrum, or poppy, for which he sometimes

substitutes inunctions with oil of *hyoscyamus*, *baume tranquille*, *cerae* and opium, or chloroform and glycerine:

B. Chloroformi, ʒj.;
Glycerina, ʒiv.

These measures, to which may be added the exhibition of mild saline aperients, often effect a prompt and rapid cure.

But when prurigo is of long standing, which is in general the case, it is necessary to have recourse to external resolutive applications, and to prescribe acidulated drinks and strict attention to diet. Alkaline lotions are extremely beneficial. Baths containing the carbonates of soda or potash promote resolution, and allay the distressing irritation of the skin. Aromatic fumigations, vapour-baths, and even baths containing corrosive sublimate, are also very serviceable. But, of all topical remedies, M. Duchesne-Duparc conceives tar in lotions or ointments, associated with opium, as by far the most effective. The tar-water lotions and baths should be frequently repeated, and the author often prescribes the following ointment, which he confidently recommends as a most useful sedative:—

B. Picis liquidæ, ʒj.;
Extr. opii, ʒij.;
Adipis, ʒvijj.

It is always desirable to add in the baths a gelatinous or farinaceous substance to the alkaline salts—two pounds of isinglass, for instance, should be dissolved in each bath; this precaution is suggested by the fact that the diseased element of the skin is the papillary layer, the most sensitive of all the anatomical constituents of the integument.

M. Duchesne-Duparc, from experience, reports unfavourably of the efficacy of sulphurous waters in prurigo. They prove beneficial in complicated cases only, when pityriasis or psoriasis are also present. He therefore recommends as most likely to be useful alkaline mineral waters, containing abundant vegetable fatty matters, such as the spas of Néris.

In *prurigo pudendi*, and in *pr. podicis*, it is often necessary to resort to embrocations containing prussic acid, sulphate or muriate of morphia, or chloroform. In some cases astringent and antispasmodics may be alternately prescribed with benefit; in others, very superficial cauterization has often greatly mitigated the local irritation, and even sometimes effected a cure.

While acknowledging the utility of local treatment in prurigo, M. Duchesne-Duparc professes that in chronic cases, when the disease has proved obstinate, and has spread over a considerable extent of the cutaneous surface, internal remedies are necessary. Aconite, for instance, is a most useful medicine, and several years ago we pointed out the advantages derived from its exhibition by M. Cazenave, as a sedative in papular affections, and more especially in prurigo. Viewing this disease as a neuralgia of the papillary structure of the cutis, and the development of papulae as a secondary phenomenon, M. Cazenave prescribed simultaneously

alkaline baths and one or two of the following pills night and morning:—

B. Extract. aconiti, }
taraxaci, } a gr. xv.

M. divide in pilulas xl.

M. Duchesne-Duparc administers pills of nearly the same description, but in general prefers the tincture to the extract, and exhibits the drug in gradually increasing doses to forty-five and sixty grains daily, in four ounces of sugar and water. In obstinate cases, arseniate of iron and protracted starch-baths are added to this treatment with the most satisfactory results. In chronic prurigo (*lichen agrius*), the baths should be of considerable duration, and last as much as two or three hours every day. If at the same time the affected parts are carefully rubbed or shampooed, the softness and elasticity of the skin are often restored in an unlooked-for manner, a remark which will generally be found true in all diseases of the integument.

ART. 69.—*On the Treatment of Itch.*

By M. HARDY.

(*Gaz. des Hôp.*, No. 67; and *Dub. Med. Press*, June 18, 1864.)

The treatment employed by M. Hardy in the cases of itch applying to the Hôpital St. Louis at Paris, consists, first, in friction for half-an-hour of the whole body, except the head, by means of black soap. This process, which is first carried on out of a bath, is afterwards continued by the patient himself during an hour while in a tepid bath. After the bath the patient is submitted to a rapid and general friction with the following ointment:—Lard 64, sulphur 20, subcarbonate of potass and water of each 8 parts. The patient then dresses himself without wiping off the ointment, as the contact of this is necessary for the destruction of any remaining acari in the skin or in the garments. Of 37,429 persons so treated in 1852-62, only 535 required a second application.

ART. 70.—*A New Theory of the Pathogenesis of Psoriasis.*

By Dr. WERTHEIM.

(*Imperial and Royal Society of Physicians*, Vienna, Dec. 11, 1863; and *Gazette Hebdom.*, Juillet 1, 1864.)

The researches and experiments of Dr. Wertheim extended over a period of two years, during which, in a very large number of cases of psoriasis, he examined, under the microscope, diseased portions of the skin. In all the cases, without a single exception, he found a marked increase in the length and breadth of the cutaneous papillæ, and a similar abnormal development of the vascular plexuses inside the diseased papillæ. This hypertrophy was a constant phenomenon,

and distinguished parasites from other malignant substances of an inflammatory nature. He has found by means of the microscope germs in the blood of the patients, while at the same time a microscope will distinguish a microorganism.

Having failed to obtain any positive result in these experiments the author, who is often in Paris, has sought to repeat his experiments simultaneously the effects of injecting in the veins of the same subjects and subjected to successive operations, a dog skin and human skin. When I say my last, that consists of the subject of the skins of the greater number of the subjects showing the parasitic newly developed fungi, according to the opinion of Pouchet, Musset, and others, and of myself, and of the experts known. As these experiments were carried on two or three months ago, it is difficult to give exact figures, but twenty patients, and twenty dogs, and twenty individuals, and twenty patients, all suffered from psoriasis. Dr. Wertheim himself acknowledges that the tumor is not sufficient large to remove entirely all the signs of tissue suppuration. The results seemed to him extremely similar. A dog was selected by Wertheim as giving the effects that would follow the entrance of these fungi into the vascular system of animals. He therefore injected into the femoral vein of several dogs, four cubic centimeters of a filtered emulsion of penicillium glaucum with distilled water. The loss of blood consequent on the injection was insigificant. Twenty-four hours afterward there appeared in the four limbs first at their extremity, then over the tarsus and the knee, numerous red patches, perfectly isolated and having the characters of inflammatory patches, whilst some of them were slightly elevated so as to resemble nodes. On the next day the malady had increased in number and extent, and by repeating the injection in the same dog, their development was rapidly increased. But what is perhaps one of the most remarkable results of Dr. Wertheim's experiments is, that he obtained identical effects by injecting an emulsion of *pusis*. The author next examined most carefully with the unaided eye as well as the microscope, the eruption he had artificially produced in the dogs. This exanthem, which presented the greatest analogy to psoriasis, both in regard to its seat, its symmetrical distribution, its form, and its progress, was found to be due to an obstruction of the capillaries of the skin by the vegetable elements injected into the veins. The anatomical preparations were submitted to the members of the Society, and the above interpretation of the facts was accepted by them. On these grounds, therefore, Dr. Wertheim concludes that psoriasis in man is produced by the entrance into the circulation of vegetable parasites. Now, the question arises how do these fungi enter the organism? Atmospheric air must be first excluded, for the precise researches of Pouchet, Musset, and Joly, have shown it to be extremely poor in vegetable germs. Cooked food, solid or liquid, must be excluded also, since a temperature of 100° C., kept up for two or three minutes, suffices for preventing the development of germs, as shown by the researches of Pasteur. There only remain, then, ailments eaten in a raw state, and fermented liquids imperfectly cleared. As ingesta answering these conditions are used by all nations and all classes of society, no wonder psoriasis

is met with everywhere. The disease is more common in males than in females (Devergie found that, in 270 cases, 245 were males and 25 females), and it is certainly true that women, as a rule, consume less alcoholic drinks than men. As yet, the only certain etiological condition known for the development of psoriasis was hereditary predisposition; and this fact, according to Dr. Wertheim, does not militate against his theory, for entozoid germs have often been known to be transmitted from parent to offspring.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

ART. 71.—*On the Destruction of Tumours by the Electrolytic Method.*

By M. NÉLATON.

(*Gaz. Hebdom.* Juillet 29, 1864.)

Although Surgery possesses a great many methods for destroying tumours, its resources are limited enough in the case of some particular tumours. Such, for instance, are tumours deeply seated in a natural cavity, intimately connected with and adhering to its walls by a broad base, with difficulty seen, not easily reached by the hand or by instruments, and which are besides extremely vascular. Nasopharyngeal polypi may be mentioned as types of this class. Too often are they seen to bleed abundantly, as soon as they are touched with a cutting instrument, and the blood falls into the pharynx, and passing down into the larynx, threatens suffocation, until it is expelled by coughing and by vomiting, whilst the surgeon, in the midst of all these convulsive movements, can scarcely distinguish the parts which he has to operate upon.

Having recognised the inutility of the ligature, of torsion, the linear écraseur, and cauterization in its various forms of application, such as the actual cautery, the galvanic and the gas cautery, chloride of zinc, strong nitric acid, &c., M. Nélaton thought of having recourse to a method of destruction borrowed from electricity.

With the assistance of one of his pupils, M. Arnould Thénard, he first made numerous experiments on living animals, with the view of studying the nature of those partial destructions of tissue which are seen round the needles used in electro-puncture. The results he obtained he sums up thus:—When two platinum needles, connected with the poles of a fully charged Bunsen's battery, consisting of 9 cells, 16 centimètres high and 8 broad, are thrust into the muscles of a living animal, there occur, after an interval of eight or ten minutes, the following modifications: round the positive needle, an indurated, well-circumscribed cylinder, from 12 to 15 millimètres in diameter; whilst around the negative needle

the tissue of the part has undergone a kind of softening over a similar space. There is in the meantime no sensible elevation of temperature, and the only phenomenon observed is the appearance round the point of insertion of the needles, of a whitish froth due to extremely fine gas-bubbles. In the substance of the modified tissue, no vessels, no trace of organization can be seen any more. All the part included within the sphere of action of the electrodes is completely modified, the modification consisting in a coagulation at the positive pole, and a tendency to liquefaction at the negative.

If the animal be not sacrificed, the phenomena which accompany the elimination of a slough develop themselves round the points acted on by the electric current. The most striking and conclusive example of the kind was that of a dog's tongue acted on by the above-mentioned current, after the two needles had been inserted at four centimètres from its extremity. There was, first, production of a slough which extended from one side of the tongue to the other, and soon after, the portion of the organ beyond the slough became shrunk and gangrenous. There were then, in this case, two different modes of destruction—an electrolytic action, and gangrene caused by an interruption in the circulation.

This method was adopted in the case of a naso-pharyngeal polypus of large size, very vascular, bleeding on the slightest touch, deeply situated in the pharynx and nasal fossæ, vainly though perseveringly attacked by the most powerful agents, and which, in six sittings, was destroyed by the insertion of 2 electrodes within its substance. No blood was lost, and the pain produced was easily borne.

ART. 72.—*On Lymphatic Tumours.*

By M. TRÉLAT.

(*Gaz. des Hôpitaux*, Juillet, 1864; and *British Med. Journ.*, Sept. 3, 1864.)

At the meeting of the Surgical Society of Paris on June 22nd, M. Trélat related the case of a young man who had come under his care with a rare disorder—tumours formed of dilatations of the lymphatic vessels. The patient was robust and of good muscular development. He was a native of the island Bourbon, which he had left for the first time in order to come to France. He had never had syphilis; there was no trace of scrofula, nor were there any enlarged glands in the neck or axillæ. When he was about fifteen years of age, a small enlargement appeared below the fold of the left groin; and soon afterwards, while performing gymnastic exercises, the patient was seized with severe pain on the right side, which was found to proceed from an inguinal hernia. This was reduced, and a truss was applied; but the region above the pad remained enlarged, especially after walking or exertion—there was, in fact, a tumour in the right groin also, independently of the hernia. The patient at first paid no attention to the malady; and could not inform M. Trélat

whether the tumours grew rapidly or slowly. He stated, however, that they had remained nearly stationary during the last four years.

When M. Trélat first saw the patient, he had an inguinal hernia on the right side. In addition, Scarpa's triangle on each side was occupied by a tumour elongated from below upwards. The tumour on the right side was 12 centimètres long by 7 centimètres broad; that on the left side was 13 centimètres by 8. The former descended somewhat lower than the other; but that on the left reached as far upwards as the internal orifice of the inguinal canal. The tumour on the right side was a little more projecting, more regular and softer. That on the left side presented several lobules, of about the size of an almond, formed by the lymphatic glands. With these slight differences, the tumours were alike on the two sides. The skin presented no change of colour; it had not that bluish tint which is observed in varix and some subcutaneous erectile tumours. The surface was regular, perfectly normal, without that orange-rind appearance of the skin which is observed in varix of the superficial lymphatic network. No lesion or alteration of any kind could be detected in the two lower limbs. The skin was perfectly moveable over the tumours, which could also be readily moved over the deeper parts. The swellings were soft, could also be compressed in every direction, and felt like lipoma, from which, however, they differed in being reducible. Another diagnostic sign was furnished by the rarity with which symmetrical lipomata exist, without the presence of other similar tumours irregularly scattered elsewhere. During about four months, the patient had complained every three or four days of vertigo, accompanied by dyspepsia; the symptoms were removed by lying down and simple treatment. This reminded M. Trélat that a patient whose case was described in 1854 by MM. Desjardins and Gubler, and who had lymphatic fistulæ, was troubled by *malaise*, nausea, and vertigo, when she had lost a considerable quantity of lymph; and that a patient seen by Amussat had severe symptoms which ended fatally. M. Trélat's patient, however, had lately been very anxious about his disorder, and this alone might be sufficient to account for the dyspepsia and vertigo. Moreover, the vertigo had occurred only once during his voyage, and not since his arrival in France; since which his digestion had been very regular, and he had been able to bear exertion more easily than in Bourbon.

M. Trélat observes, that lymphatic varix appears especially frequent in warm countries. Thus, Amussat's patient was from the island of Bourbon; that of MM. Desjardins and Gubler from the Mauritius. Of four patients observed by M. Nélaton, two were originally from Brazil or the colonies; and Drs. Saint-Perne and Petit, who have practised in Bourbon, have informed M. Trélat that of several instances of lymphatic tumours in natives of that region, the patients were mostly young, the ages varying from seventeen to twenty-three, one only was thirty-nine years of age.

M. Trélat did not entertain the idea of removing the tumours, and in this he was supported by the experience of M. Nélaton. Some years ago M. Nélaton, at the urgent request of a young man, decided on excising one of these lymphatic tumours. The diagnosis being

uncertain, an incision was made over the mass, when a considerable quantity of thickish milk-like fluid escaped, leaving only irregular flaps, and some beaded filaments which were removed. The patient, a young robust man, was soon seized with rigors and symptoms of purulent absorption, and died. The tumour on the opposite side, which had not been operated on, was injected with mercury by M. Sappey, and was shown to consist of a network of varicose lymphatic vessels.

Compression was applied in M. Trélat's case by one of Bourjeard's bandages, which was, however, obliged to be modified several times to meet the difficulty experienced in keeping up pressure. This was the only treatment that had been employed, and M. Trélat was desirous of learning from his colleagues in the Society, whether it would be justifiable to attempt to evacuate the contents of the swelling through an artificial fistulous opening. Some observations, principally bearing on the diagnosis of lipoma, were made by MM. Verneuil, Morel-Lavallée, Guyon, and Larrey, and the unanimous opinion was expressed that operation was not advisable.

ART. 73.—*On the Efficacy of Chlorate of Potash in the Treatment of Epithelioma.*

By M. BERGERON.

(*Journ. de Méd. et Chir. Prat., Fév., 1864.*)

In this paper (which was read before the Academy of Medicine, at Paris), M. Bergeron attempted to show—

That cancroid growths of the skin and mucous membrane of the mouth, the nature of which was in several instances clearly ascertained by microscopic examination, have been undoubtedly cured by the persevering exhibition of chlorate of potash during a period of two months at least, or of four, five, and six months at most.

That in one case observed by M. Milon, and in another which occurred in the author's own practice, a complete cure was obtained exclusively with lotions and fomentations, with a solution of the chlorate; and that the efficacy of the external treatment has again been since illustrated in the most peremptory manner in a patient treated by Dr. Blondeau.

That, as far as it is possible to judge at present, local applications are more efficient than the internal administration of the remedy.

That the chlorate should nevertheless be exhibited in cancroid of the rectum and uterus; in the first place, because in general local applications are not impracticable, and also because in the case of a patient at present at La Salpêtrière, cancroid growths have been all but cured, merely by the use of chlorate of potash: it thus appears that in time the effects of the drug may become apparent in regions far removed from the absorbing surfaces.

That in his practice, M. Bergeron has employed a solution con-

taining one twenty-fifth part of its weight of chlorate of potash, and in his earlier patients merely applied the liquid with a brush, night and morning, to the surface of the cancroids. But the rapidity of the cure in M. Blondeau's case, suggests a hope that a more concentrated solution permanently placed in contact with the diseased surfaces, may lead to more prompt results.

That the internal treatment, consisting in the daily exhibition of half a drachm of chlorate in four ounces of water, divided into five or six doses, was borne with perfect ease for nearly four successive months by two patients of La Salpêtrière.

That the same medication induced, on the contrary, symptoms of dyspepsia, after a fortnight, in two cases under the charge of M. Laugier and of M. Léger, and that it was necessary for a time to discontinue the treatment.

That in a patient under the care of M. Devergie, the gastric disturbances were so severe as to compel him to cease the medication altogether, and that it is, therefore, prudent to begin with a small dose (ten or fifteen grains daily, for instance), which can, if necessary, be subsequently increased.

**ART. 74.—On the Treatment of Scrofulous Ulcers with
Tinctura Cantharidis.**

By Dr. JOHN SHORT, Zillah Surgeon.

(*Indian Annals of Med. Sci.* April, 1864.)

In the following case the patient was for nearly fifteen months under treatment, and during that period almost every variety of general and local treatment was resorted to without success. In five weeks from the commencement of the cantharidis treatment, the patient was cured and able to return to duty. The cicatrices are firm and sound, and the patient has enjoyed the best of health ever since.

CASE.—Kistnareddy, aged forty-five, a convicted prisoner, sentenced to fourteen years' imprisonment—was admitted into the gaol hospital on the 15th February, 1858, with a couple of scrofulous looking sores, each the size of a shilling piece, situated on the left scrotum, one at the root of the penis, and the other two inches below it. The sores are unhealthy, foul, and excavated, with more or less irregularity of the edges. The general health is stated to be good, but the patient has apparently lost flesh, and looks weak and anaemic, with an oldish appearance. States that he has had these sores for the last four months, and that they have given him much pain, worse especially at night. Is not aware of ever having suffered from syphilis. Functions natural. He was ordered a castor oil purge, and water dressings to sores. To have the following pills thrice daily :

B Pil. hydrarg. gr. i.
Pulv. rhei, gr. iiij.
Opii, gr. $\frac{1}{2}$. Ft. pil.

March 15th.—The ulcers are stationary. An abscess formed in the

scrotum which was opened, and about 3ij. of curdy pus let out. The following draught was substituted for the pills :

R. Potass. hydriodat. gr. v.
Decoc. cinchonæ, 3ij.

This draught to be taken three times daily. Stimulant dressings to sores.

April 15th.—The original ulcers are stationary, and a few sinuses have formed at the base of scrotum ; these were opened out with a bistoury, and the following pills substituted for the cinchona and iodide of potass draughts :

R. Pil. hydrarg. gr. iii.
Ferri sulph. gr. i.
Opii, gr. $\frac{1}{2}$. Ft. pil. To be taken thrice daily.

Black wash at first, and afterwards nitric acid lotion was applied to the sores.

May 15th.—The patient has improved in health and flesh. Sores are granulating freely, promising to do well, gums tender, omit pill. To have half a grain of opium three times daily. Simple dressings to sores.

June 15th.—The ulcers have returned to their unhealthy state ; they improve in character for a few days, and then degenerate into foulness. The opium was changed for ten grains of the hydrocotyle Asiatica thrice daily, and red precipitate ointment dressings subsequently. Tinct. of iodine lotion, of the strength 3j. to 5viii.

July 15th.—One or two of the sores have healed, whilst the others are as foul as ever, and are corroding into the flesh. The patient's health is excellent, and he has become quite stout. The hydrocotyle was changed for 3ij. of decoct. cinchonæ thrice daily, and charcoal poultice to sores at first, and lint steeped in the muriate of iron subsequently.

August 15th.—No impression made on the character of the sores ; one is about the size of a halfcrown piece, and nearly an inch deep. A grain of opium was substituted for the cinchona, at first, and changed for 10m. of the tinct. iodine, to be given in an ounce of infusion chiretta thrice daily, and a grain of opium at bed-time, with lint dressings.

Sept. 15th.—No improvement in the character or appearance of the sores ; they have extended up to the groin. The medicines were changed for the solution of the bichloride of mercury, beginning with 20, and increased to 40 minims, with 4 ounces of the compound decoction of sarsaparilla given thrice daily. Ungt. hydr. nitrat. dressings to sores.

October 15th.—None of the remedies tried hitherto appeared to have made any impression on the sores. Fish oil was substituted for the solution of bichloride of mercury, and poultices to sores after completely destroying their surface with pure nitric acid.

November 15th.—On the removal of sloughs caused by the application of the nitric acid, a clean, healthy, granulating surface was obtained. The following was substituted for the fish oil :

R. Pot. iodid. gr. v.
Quinæ disulph. gr. ii.
Decoc. sarzæ co. 5iv. Ft. haust.

To be taken three times a day. Apply adhesive strapping to sores.

December 15th.—The sores healed, but fresh ones opened out at their edges, attacking new skin, and sinuses formed requiring to be laid open.

The pure nitric acid was tried again without benefit. The patient was now placed under the influence of chloroform, and the diseased skin of the

left side of the scrotum from a little beyond the raphe, and that on the right side was cut away by the knife, as also every part that appeared diseased.

The anterior surface of the left testicle was indurated. That portion of the gland was also sliced away, and the remnant of the skin of the scrotum was drawn over, and fixed by silver sutures. No difficulty was experienced in finding sufficient cover for the gland. The usual water dressings were applied to the wound.

January 15th, 1859.—The wound healed by the first intention, but, to my confusion, just as I was about returning the patient to duty, fresh sores appeared over the penis, exhibiting the same characters, having irregular jagged edges, scooped out centres, and foul surfaces, with a tendency to spread. The solution of bichloride of mercury with cinchona mixture was again had recourse to, and the warm unguentum resinæ dressings applied.

February 15th.—No improvement; the ulcers are slowly extending; all internal medicines were omitted, and balsam of Peru with lint applied to the sores.

March 1st.—Although the scrotum continued sound, the ulcers over the penis continued to extend, and were now about the size of a crown piece, scooped out in the centre, with white thickened irregular edges. The following draught was ordered :

R. Tinct. cantharidis, m. x.
Decoct. cinchonæ, 3ij. Ft. haust.

To be taken thrice daily. Cold water dressings.

An improvement in the character of the ulcers was perceptible in the course of two or three days, and on the 6th of April the patient was discharged cured.

It will thus be seen that, after a variety of treatment for nearly thirteen months without success, the patient was thoroughly cured in five weeks after the tinctura cantharidis was commenced.

I believe we are indebted to Mr. Tait of the Madras Medical Service for first drawing attention to the use of tinct. cantharidis in ulcers exactly similar to the above, occurring in the Straits. It is now three years since the patient in this case was cured, and he has never had a return of the disease; on the contrary, he continues to enjoy uninterrupted good health. I am happy to say that I have tried it in several cases of the kind since, with the same unvarying success.

ART. 75.—*On Extemporaneous Ligature.*

By M. MAISONNEUVE.

(*Journ. de Méd. et Chir. Prat.*, Avril, 1864.)

The following remarks are from a report of some points of practice in M. Maisonneuve's wards at the Hôtel Dieu, Paris. The name of the reporter is not given :—

" The simplicity of the means of constriction resorted to by M. Maisonneuve invests them with a certain superiority over M. Chassaignac's powerful écraseur; they are readily procured, and may, in a certain degree, permit the surgeon to dispense with so costly an instrument. In addition, when the field of operation is limited in extent—for instance, in the removal of polypus of the ear, of pterygium, and a host of small pediculated growths, situated on the

genital organs or elsewhere—a piece of silver thread or wire, a silk or thread ligature, are more easily managed than a metallic chain-saw, endowed with mobility in one direction only, and which cannot readily embrace tumours included in narrow cavities, or lying on irregular surfaces. MM. Matthieu and Lüer have, it is true, constructed on the same plan as the larger instrument very small écraseurs, which may be carried in a pocket-case ; but their use is not general, and it is desirable that the humblest practitioner should be enabled to impart to his patients the invaluable benefits of a method founded on a principle which is not new, but the regular application of which to modern surgery gives to M. Chassaignac a strong claim on the gratitude of the profession.

"M. Maisonneuve remarks, in the first place, that in many instances no instrument whatever is necessary, and that the surgeon can apply all the constriction required with his hand or fingers. Simple metallic wire is often sufficient for the excision of vegetations or small polypi. In general, however, some sort of mechanism must be superadded, the ligature-adjuster, for instance, which consists merely in a metallic tube containing the double ligature ; one of the extremities of the tube forms with the loop a circle, in which the tissues are included and crushed, whilst the tractive efforts are applied at the other end. In most cases a female catheter will answer the purpose ; a small winch may be added, on which the extremities of the ligature can be coiled so as to tighten the constriction at will. Gräfe's ligature-adjuster may also be used ; it consists of a metallic tube, including a screw and ring, to which is attached the ligature, and which in receding tightens the loop, so as to cut through any intervening substance,

"M. Maisonneuve's pocket-ligature-tighteners, which he uses for the amputation of polypi of the nose and ear, the excision of the uvula, and of haemorrhoids, for the incision of fistula in ano, and the removal of every kind of pediculated growth, closely resemble Gräfe's instrument, with this difference, that the extremity from which issues the loop is flattened instead of being round, so as to form a narrow slit, and not a circular aperture. The constriction is generally effected with annealed iron wire, one-third of a line in diameter. This very simple contrivance might be found sufficient in most cases, and fully deserves a corner in every surgeon's instrument-case.

"If greater power is required, M. Maisonneuve has recourse to his constricteur, an instrument constructed on the same principle, but on a much larger scale.

"We shall now describe two applications of extemporaneous ligature.

"The first case which occurs in our notes is the excision of an epithelioma of the lower lip and left commissure, in a man of sixty. The disease occupied the entire length and depth of the lip, and on the 6th of February M. Maisonneuve performed the operation, with a piece of common packthread. As the growth reposed on a broad basis, the surgeon resorted to the arch ligature (*en arcade*), which Récamier has recommended for the extirpation of tumours of the tongue, and of annular cancer of the rectum. With a ligature-needle,

three strong threads were inserted through the skin at an inch from each other, and each thread was then used as a conductor to pass through the tissues a loop of strong twine. The loops were then cut, and each extremity securely tied to the neighbouring ligature, forming a series of arches which involved the entire base of the scrotum; the free extremities of the twine were secured in the same manner, and passed through the tube of the constrictor. The growth was thus detached without any hemorrhage whatever in less than five minutes, and no dressing was applied to the wound, which at first was perfectly dry. Suppuration, however, soon set in, and on the thirteenth day the patient was discharged entirely cured.

"We noticed another case: the patient was a man affected with suspicious disease of the testicle. Chloroform was exhibited, and the testis was removed by ligature. A loop of iron wire was passed round the testis, skin, and spermatic cord: the scrotum was set in motion, the loop was tightened to the extreme limits of constriction, and the envelopes and cord were cut through without the escape of a single drop of blood. The wound, which was extremely small, was dressed, as it is usual in M. Maisonneuve's wards, with the glycerine and phenol liniment (phenic acid, 1 : glycerine, 99), and on the sixth day after operation the lips of the incision were united with *serrés-jânes*. In the course of three weeks a complete cure was effected."

ART. 76.—*On Chloroform.*

By the COMMITTEE of the Royal Medico-Chir. Society
on Chloroform.

(*Proceedings of the Royal Medico-Chirurgical Society, May 17, 1864.*)

In laying their report before the Council of the Medical and Chirurgical Society, the Committee on Chloroform desire to state they have made comparatively little reference to the medical portion of the subject. This is not due to their thinking the medical uses of chloroform of little importance, but to the fact that but few replies to their inquiries upon this point have been received.

In view of the great extent of the subject submitted to their consideration, the committee directed their attention to such points as appeared to them of chief practical importance. Thus their observations respecting the action of chloroform on the nervous system, and their remarks on some other points, are less full than would have been desirable had the committee regarded such details as of equal importance with those especially elected for investigation—such as its influence on the action of the heart and on respiration.

The committee have chiefly confined their physiological report to observations which they have themselves made. Without overlooking or neglecting the labours of former investigators, they have endeavoured rather to furnish an accurate account of experiments

which they have observed carefully and together, and to compare the results thus obtained and agreed upon, with the phenomena of cases in which death or peril of life has arisen from the inhalation of chloroform in the human subject.

Physiological Conclusions.—The sequence of the phenomena produced by chloroform inhalation in animals is similar to that observed in man, and if the same per-cent-age of the agent be administered the results produced are nearly uniform. The first effect of chloroform vapour is to increase the force of the heart's action ; but this effect is slight and transient, for when complete anæsthesia is produced, the heart in all cases acts with less than its natural force. The strongest doses of chloroform vapour, when admitted freely into the lungs, destroy animal life by arresting the action of the heart; whilst by moderate doses the heart's action is much weakened for some time before death ensues, respiration generally, but not invariably, ceasing before the action of the heart, death being due both to the failure of the heart's action and to that of the respiratory function. The danger attending the use of chloroform increases with the degree of stupor it induces ; the apparent irregularities in the action of the anæsthetic mainly depending on the varying strength of the vapour employed, on the quality of the chloroform, and on the constitution of the patient. In order that it may be administered with comparative safety, it is necessary that the proportion of vapour should not exceed $3\frac{1}{2}$ per cent., that its effects should be carefully watched, and the inhalation suspended when the required anæsthesia is induced.

In many respects the action of ether is similar to that of dilute chloroform. At first its vapour increases the force of the heart's action—an effect which is both greater and of longer duration than that observed with chloroform. The stimulation is followed by a depression of the force of the heart's action, but at the same degree of insensibility ether does not depress the action of the heart to the same extent as chloroform. Eventually ether kills partly by enfeebling the action of the heart, but chiefly by arresting the movements of respiration. Thus the energy with which chloroform acts, and the extent to which it depresses the force of the heart's action, render it necessary to exercise great caution in its administration, and suggest the expediency of searching for other less objectionable anæsthetics. Ether is slow and uncertain in its action, though it is capable of producing the requisite insensibility, and is less dangerous in its operation than chloroform. On the whole, however, the committee concur in the general opinion which in this country has led to the disuse of ether as an inconvenient anæsthetic.

A mixture of ether and chloroform is as effective as pure chloroform, and a safer agent when deep and prolonged anæsthesia is to be induced ; though slow in its action, it is sufficiently rapid in its operation to be convenient for general use. A mixture composed of three parts of ether, two parts of chloroform, and one part of alcohol (by measure), is to be preferred on account of the uniform blending of the ether and chloroform when combined with alcohol, and the equable escape of the constituents in vapour ; and the committee

should not be given to those elements of the drug which are likely to be injurious.

For example, if the patient has a history of heart trouble, it would be unwise to administer a preparation containing digitalis, because this drug is a powerful stimulant to the heart. If the patient has a history of kidney trouble, it would be unwise to administer a preparation containing a strong diuretic.

Preparations containing opium or morphine should not be given to patients who have a history of chronic constipation, because these drugs are powerful stimulants to the bowels. If the patient has a history of chronic bronchitis, it would be unwise to administer a preparation containing a strong expectorant, because this drug may irritate the mucous membranes of the respiratory tract. If the patient has a history of chronic gastritis, it would be unwise to administer a preparation containing a strong antacid, because this drug may irritate the mucous membranes of the stomach.

Remember—the best way to give a drug to a patient is to give him the smallest quantity of the drug which will produce the desired effect. If a drug is given in a quantity which is too large, it may produce undesirable side effects. For example, if a drug is given in a quantity which is too large, it may produce a toxic reaction. If a drug is given in a quantity which is too small, it may not produce the desired effect. Therefore, the best way to give a drug to a patient is to give him the smallest quantity of the drug which will produce the desired effect.

Examination of the patient before giving a drug.

The physician should try to ascertain the general condition of the patient—whether he is well or ill, whether he is fatigued or not, whether he is able to walk or not, etc. This information should be taken into account when giving a drug to the patient.

Under no circumstances is it desirable for a person to give a drug to himself.

It is not advisable to give an anesthetic after a long walk or swim after a meal, or the best time for its administration being soon after our hour after food has been taken.

If the patient is much depressed, there is no objection to his taking a small quantity of brandy, wine, or ammonia, before commencing the intravenous.

Provision for the free admission of air during the patient's anæsthesia is absolutely necessary.

The recumbent position of the patient is preferable, the prone position is inconvenient to the administrator, but entails no extra

danger. In the erect or sitting posture there is danger from syncope. Sudden elevation or turning of the body should be avoided.

An apparatus is not essential to safety, if due care be taken in giving the chloroform. Free admixture of air with the anaesthetic is of the first importance, and guaranteeing this, any apparatus may be employed. If lint, or a handkerchief, or a napkin is used, it should be folded as an open cone, or held an inch or an inch and a half from the face.

The anaesthetic should invariably be given slowly. Sudden increase of the strength of the anaesthetic is most dangerous. Three and a half per cent. is the average amount, and four and a half per cent. with ninety-five and a-half of atmospheric air is the maximum of the anaesthetic which can be required. Given cautiously at first, the quantity, within this limit, should be slowly increased according to the necessities of the case, the administrator being guided more by its effect on the patient than by the quantity exhibited.

The administrator should watch the respiration of his patient, and should keep one hand free for careful observation of the pulse.

The patient who appears likely to vomit whilst beginning to inhale the anaesthetic should be at once brought fully under its influence, and the tendency to sickness will then cease.

The occurrence during the administration of an anaesthetic of sudden pallor or of sudden lividity of the patient's countenance, or sudden failure or flickering of the pulse, or feeble or shallow respirations, indicates danger, and necessitates immediate withdrawal of the anaesthetic until such symptoms have disappeared. On the occurrence of these symptoms, and especially if they should become so urgent as to threaten death from failure of respiration, of heart-action, or of both together, the following rules of treatment are to be observed:—Allow free access of fresh air; pull forward the tongue, and clear the mouth and fauces; keep or place the patient recumbent; dash cold water on the face and chest, and aid the respiratory movements by rhythmical compression of the thorax. In the more threatening cases artificial respiration must be commenced instantly; and this rule applies equally in all cases, whether the respiration has failed alone, or the pulse and respiration together. Galvanism may be used in addition to artificial respiration, but the artificial respiration is on no account to be delayed or suspended in order that galvanism may be tried.

Few if any persons are unsusceptible of the influence of chloroform, from two to ten minutes being required to induce anaesthesia. The time, however, varies with age, temperament, and habits.

The mixture of chloroform, ether, and alcohol should be given in the same way as chloroform alone; care being taken, when lint or a handkerchief is used, to prevent the too free escape of the vapour.

Use of Chloroform in Surgical Operations.

With heart-disease the anaesthetic may be given in any case which requires an operation, although, when there is evidence of a fatty,

weak, or dilated heart, great caution is demanded. Tabescent disease is of less importance.

In phthisis, when an operation is unavoidable, the anesthetic may be given with impunity.

For all operations upon the jaws and teeth, the lips, cheeks, and tongue, the anesthetic may be inhaled with ordinary safety. By care and good management the patient may be kept under its influence to the completion of the operation. In these cases, blood, if it escapes, if not vomited by the mouth, passes into the pharynx. If very small quantity finds its way through the larynx, it is readily expelled by coughing. In operations upon the soft palate, fauces, pharynx, and posterior nares, if sudden or severe hemorrhage is likely to occur, it is not advisable to induce deep insensibility.

In cases requiring laryngotomy and tracheotomy the anesthetic may be employed with safety and advantage.

For operations upon the eye, involving the contents of the globe, the use of anesthetic is open to objection, on account of the damage which the eye may sustain from muscular straining or vomiting. If employed, profound insensibility should be induced.

In operations for hernia, and in the application of the tourniquet, the anesthetic acts most beneficially. For most operations about the nose, profound anesthesia is positively demanded.

In the condition of shock, or of great depression, as after hemorrhage, careful administration of the anesthetic diminishes the risk of an operation.

In all cases other than those specially referred to, it is sufficient to state, so far as the mere surgical operation is concerned, that an anesthetic may invariably be administered.

The continuous vomiting occasionally induced by and following upon the inhalation of anesthetics, may be injurious by consequent exhaustion, as well as by mechanically disturbing the repair of a wound. With this reservation, they do not appear to interfere with the recovery of patients from surgical operations.

Statistics.—The results of 2586 capital operations performed before, and of 1860 performed since, the introduction of anesthetics, collected from all authentic available sources, prove that anesthetics have in no degree increased the rate of mortality.

Use of Chloroform in Obstetric Practice.

(a) *In Natural Labour.*—The careful administration of chloroform during labour is not attended with special danger, there being, either in this country or abroad, so far as is known to this committee, no well-authenticated instance of sudden death where it has been given by a medical practitioner; but the occasional occurrence of unfavourable symptoms demands the exercise of caution during its employment. Administered in a moderate degree, it does not, as a rule, weaken the expulsive powers, and is decidedly beneficial in promoting dilatation of the maternal passages. It does not predispose to puerperal convulsions or other like complications. The balance of opinion is nearly equal as to whether it predisposes to imperfect contraction of the uterus after delivery. As a rule, it in

no way retards the convalescence of the mother ; nor has it any tendency to interfere injuriously with the function of lactation ; nor has it any injurious influence on the child.

(b) *In Abnormal Labour.*—The anæsthetic may be employed with advantage in various obstetrical operations—as forceps, turning, craniotomy, and extraction of retained placenta,—unless the patient is much enfeebled by haemorrhage ; when, if given, it ought to be accompanied by the use of stimulants. It may also be employed advantageously to check the paroxysms in puerperal convulsions.

(c) *As to the Preference of Ether. Rules relating to the Administration of Chloroform.*—There are no reasons for giving preference to ether over chloroform, the latter being much more desirable in obstetrical practice generally, the only exceptions being those in which chloroform notably disagrees.

In addition to those given for its administration in ordinary cases, it is generally desirable to observe the following rules during its administration in labour, subject to modifications at the discretion of the practitioner :—In natural labour, begin to give it generally at, or after, the termination of the first stage ; but it may be given earlier if the first stage is unduly painful, or if the os uteri resists dilatation. Give it only during the pains, and withdraw it in the intervals. Withdraw the chloroform immediately after the child is expelled. If the patient is depressed, or the pains are sluggish during its administration, an occasional stimulant may be administered. In cases where it seems to interfere with the progress of labour, it may be necessary to suspend its use for a time, and re-apply it after an interval, or even to withdraw it altogether. In turning and instrumental deliveries, deep anæsthesia must be induced, as in surgical operations ; and the administration should then be intrusted to a competent person, whose sole duty should be to attend to it. In midwifery a special inhaler for its administration is not generally necessary or desirable, a handkerchief or towel, folded so as to prevent blistering of the face, and to allow free admixture of atmospheric air, being sufficient for the purpose.

(d) *Use of Chloroform in Diseases of Women and Children.*—In the treatment of the diseases of women, chloroform may be employed to facilitate and lessen the pain of certain examinations. In cases of spurious pregnancy and phantom tumours, by relaxing the abdominal parietes, it may assist in demonstrating their true characters ; and, acting in the same way, it may help the practitioner to define more accurately the character and relations of other abdominal and pelvic tumours, or to detect feigned disease.

As a therapeutic agent, its inhalation and external application in the form of a liniment, may be usefully employed to allay pain in some cases of severe dysmenorrhœa, neuralgia, and the like.

There is accumulated testimony in favour of chloroform inhalation proving serviceable in various spasmodic diseases of women and children : as whooping-cough complicated with convulsions, spasmodic croup, epileptic seizures, and some other forms of convulsion in children ; hysterical convulsions, epilepsy, and various muscular contractions in women.

SECT. II.—SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 77.—*On the Employment of Subcutaneous Injections in Ophthalmic Surgery.*

By Professor VON GRAEFE.

(*Bull. Gén. de Thér.*; and *Edin. Med. Journ.*, July, 1864.)

Professor Von Gräfe lately delivered a series of clinical lectures on the employment of subcutaneous injections in ophthalmic surgery, of which we propose to give a brief abstract. His experiments have been only made with the acetate of morphia and the sulphate of atropine. The most favourable situation for making the injections is the temporal region, and it is this which the Professor chooses under all circumstances, unless there be some special indication, such as neuralgia or spasmodic phenomena, which makes it probable that some other point may be preferable. The integument should be well raised from the subjacent parts—the canula should be pushed into the cellular tissue, and the skin should be closely applied around the canula, so as to prevent the return of the liquid injected. The quantity of acetate of morphia employed in Gräfe's experiments varied from the tenth of a grain to half a grain—a fifth or sixth of a grain being the usual quantity. The solution contained four grains of the acetate in a drachm of distilled water; it should be neuter, or very feebly acid. The physiological action is the same as when morphia is taken into the stomach, but in general it is better marked, and consequently the amount injected ought to be smaller by about a third than the quantity which would be administered internally. The action on the iris is interesting. Often at the end of a minute, sometimes not for half an hour, the special contraction of the iris (*opium-myosis*) manifests itself. This contraction is best observed by comparing the dimension of the pupil with a moderate light. The degree and the duration of the myosis vary remarkably; in a large number of cases it remains well marked for several hours, and disappears slowly. Sometimes, in very irritable persons, spasm of the muscle of accommodation of the iris takes place; when this phenomenon occurs, it is at an advanced period, at the end of the stage of irritation. The most important therapeutical indications of subcutaneous injections of morphia are, according to Gräfe, the following:—1st. In the case of traumatic injuries which have involved the eyeball, soon after their occurrence, and when there is severe pain; for instance, after the penetration of foreign bodies, superficial burns or wounds, the pain is more speedily allayed by subcutaneous injection of morphia than by the instillation of solution of atropine between the eyelids. Professor Gräfe is opposed to the application of leeches after the extraction of foreign bodies, after contusions, and after penetrating wounds; he looks upon them as more likely to produce than to prevent inflammation and suppuration. 2nd. After operations on the eye, when they are followed

within a short time by intense pain. 3rd. In the neuralgia of the ciliary nerves which accompanies iritis, glaucomatous choroiditis, and several forms of inflammation of the cornea. 4th. As an antidote for poisoning by atropine, an action which was pointed out by Mr. Benjamin Bell in 1857. 5th. In neuralgic affections of the terminal branches of the fifth pair in the frontal region, not dependent on an affection of the eye. 6th. In different forms of reflex spasms, such as spasm of the eyelids in traumatic keratitis, and spasmodyc contraction in the course of the facial nerve.

In the case of injections of atropine, the greatest prudence is necessary. In some persons the sixtieth of a grain is sufficient to give rise to general symptoms. In general the first dose injected should not exceed that quantity; it may afterwards be gradually increased to the twentieth of a grain. According to Professor Gräfe, the occasions for the employment of atropine in injections are very limited, and to produce the mydriatic effect, the form of instillation is preferable. Even when a full quantity is injected, the dilatation of the pupils is moderate, and the power of accommodation of the iris is not superseded, whilst the desired effect is obtained by much smaller doses introduced between the eyelids. In neuralgia, injections of atropine do no good—in spasmodyc affections their effect is very doubtful; so that their employment seems to be limited to cases in which the conjunctiva would not tolerate the presence of the atropine.

ART. 78.—*On late Modifications in the Operation for Extracting Cataract.*

By MR. ERNEST HART, Ophthalmic Surgeon to St. Mary's Hospital.

(*Lancet*, June 4, 1864.)

In some clinical remarks on this subject, Mr. Hart says:—

"The methods of extraction have been modified in various ways of late, and there are some new points which deserve your particular attention, and which I will clinically illustrate by reference to the cases before us.

"I have detained, that you may examine him, J. T.—, aged fifty-five, for whom I removed a cataract some months since (Feb. 1863) by the ordinary flap extraction, and who has called to show himself. He has a similar cataract now pretty fully formed in the left eye, and on which we will operate presently, which will illustrate the various parts very well. You see it is a hard, amber-grey, striated cataract, of considerable size, and dense throughout. His pupil acts well; his general health is good; he has excellent quantitative perception of light in the cataractous eye, and reads well with convex glasses with the eye operated on. Here, then, is a good case for simple extraction by flap. I retain my preference for the inferior section in flap extraction; and I observe with satisfaction that Von Gräfe, after six years' exclusive use of the superior section, has,

from independent observation, recently declared himself in favour of the inferior section, and almost in corresponding words to those which I used three years since in advocating it. Extraction by flap is the means of operation *par excellence* which gives the most beautiful and perfect results in simple cases of hard cataract. I advise you to use forceps for fixing the eye (France), employing them, however, with care; rolling the eye into place and just keeping it there without force, and never pulling at it or attempting to drag it forwards: the assistant who tears the conjunctiva with the forceps in holding the eye steady gravely compromises the success of the operation, for such a contused wound is a source of pain and irritation which may propagate itself very considerably and mischievously, as I have seen to my cost. Many of the cataract knives supplied in operating cases are too large and clumsy, and especially too broad at the base. Choose the smallest and slenderest knife that will make a sufficient section; and I think you will do well to make the incision wholly corneal. Before completing the section let your assistant release the eye and relax his hold upon the eyelid; finish the section very leisurely and very gently, so as to avoid anything like a jerk, and to diminish by all means possible the tendency to spasm of the eyeball. Let your patient rest a short time before you attempt to perform the section of the capsule with the needle, and avail yourself as much as possible of his assistance by directing him to look in the required direction; but if he be indocile or weak, rather than introduce the needle 'in the dark,' without perfectly watching and guiding its movements, let the eye be now again gently and firmly rolled into position as before. Divide the capsule freely, and you will thus avoid many mishaps from remnants of opaque capsule in the field of vision; but do not let your needle break up the lens. The expulsion of the lens requires similar care, gentleness, and decision. If you find unexpectedly a soft vitreous, and see the lens fall back instead of advancing into the lips of the wound, beware of using any of the hooks or spears which you will find in your cases, and which you are advised to use in these instances; but gently introduce your scoop behind the lower lens, and you will then have no difficulty in lifting it forward. It is difficult to fix a hook or a spear in a loose floating lens, and you are likely to do harm with them.

"Extraction well performed is an operation so brilliant, so successful, and so satisfactory, that I cannot suppose it will ever be superseded. It has been proposed to superadd in all cases excision of the iris, either as a preparatory operation some weeks before extraction (Moreen), or simultaneously with extraction (Jacobson). But I think we must require further evidence before we consent to accept this as a universal method. In the ordinary cases of hard cataract I should regard it as an unnecessary mutilation. I cannot think our patient here, for instance, would have been better off if we had mutilated his iris, which is now perfect and acts well. On the other hand, a sufficiently strong case has been made out to prove that iridectomy combined with extraction is of value in averting the effects of bruising the iris; in dealing with a hard and irritable eye;

in averting prolapse ; or in operating where the iris does not dilate well under atropine in preparing for the operation.

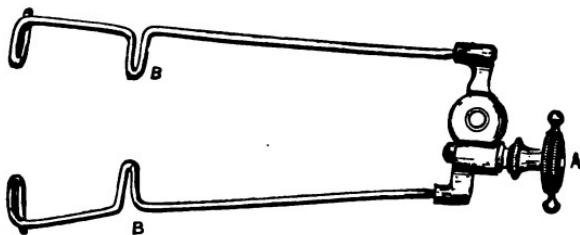
" The combination of excision of the iris with extraction by flap, is a proceeding which, according to our present knowledge of it, I hold in reserve for occasional use. You will remember two cases in which I employed it during the last three months. In the one case, because the iris was sluggish, intractable, and did not dilate well under atropine, although the case seemed otherwise favourable ; and here I feared prolapse ensuing, performed a preliminary iridectomy six weeks before the extraction, and extracted through the artificial pupil. In this case I performed the upper section, having made an upper iridectomy. In the second case there was a great deal of neuralgic pain about the eyeball, which was tense and irritable, and suffering when exposed to strong light, although the cataract was dense. There were semi-glaucomatous symptoms, and I thought it wise to perform a preliminary iridectomy. This was, perhaps, hardly a case which comes directly under the category of ordinary extractions by Moreen's procedure ; but I am not sure that I should not have been satisfied to extract directly but for the previous study of Dr. Moreen's excellent paper, and so would place the good result to his credit.

" Preliminary or associated iridectomy is, then, a means of securing additional success to the operation of extraction by corneal flap, and is well deserving of your study ; and though I believe its application will be limited, yet within those limits it is most valuable, for it is applicable to cases which were precisely the most obnoxious to failure formerly.

" But there is another mode of extraction which you have now seen me perform in many instances, and which is capable of rendering great service, and likely, I think, to be more generally applied than it has yet been : I mean linear extraction, of which we have an example in the boy B—.

" Pay especial attention to this proceeding, for it is one which we shall hope to assist in working out here clinically, and of which we have some very satisfactory examples ; it has not yet received much notice in our text-books. Linear extraction is a method deserving all your attention. It was originally proposed by Gibson of Manchester, and has recently been studied with care and great advantage by Jäger, Von Gräfe, and Schüft. I need not here attempt an historical sketch of the modifications which the operation has undergone in their hands. As I now practise it the steps are these :— The eyelids are opened by my screw speculum. This is a modification of Kelly Snowden's well-known spring speculum, and I designed it to avoid the painfully excessive distension which the instrument causes, and to enable you to open the eyelids firmly and gently, and retain them open to the required extent without pressure on the eyeball. The movement by which it is opened and closed is simple and effective, and reflects credit, I think, on Mr. Blaise, who has successfully fulfilled the indications with which I furnished him. For operations on strabismus, and for a number of operations about the eye where an automatic speculum is required, it will be found

useful. To meet with a good automatic speculum which does not exert pressure on the eyeball or excessively dilate the rima of the eyelids has been a desideratum. This instrument fulfils it well—far better than the more cumbrous and complicated spring-rack speculum lately introduced from Vienna, or even than the very ingenious stop speculum of Mr. Bader lately made by Weiss and Sons. Having introduced the speculum, you may now fix the eyeball, *rolling* it into a convenient position so as to expose well the portion of cornea which you mean to excise. Then either in the upper or external part of the cornea, close to its sclerotic edge, you introduce a lanceolar knife, of course entering nearly perpendicularly; and when the cornea is penetrated, passing on the knife in a direction parallel to the iris. If now you have ascertained that you have a fluid cataract to deal with, you may pass the lance directly onward, and incise with it boldly the capsule; then partly withdrawing it, and pressing gently and flatly backward so as to make the wound gape, nearly or even quite the whole of the fluid cataract will flow out, or only a small quantity of soft matter will remain, which is easily removed with a small curette, or with a scoop. This is the very simplest form of linear extraction. It may be employed with great advan-



tage in a number of cases for which repeated operations for solution by needles are commonly performed. The immediate disturbance produced is not greater, but rather less. There is no risk of inflammation from lodgment of a quantity of soft matter in the anterior chamber, or of pressure on the iris and consequent inflammation and injury by reason of the considerable swelling of the lacerated lens. It is especially applicable to traumatic cataract, and to the milky-blue fluid cataracts, either of congenital formation, or which, from congenital predisposition, occur in early life.

"We take care to learn beforehand what is the character, structure, and consistence of the cataract by direct (ophthalmoscopic) and oblique illumination. If, then, we have a cataract soft, but not fluid, having, it may be, a somewhat dense nucleus (which will then have a deeper colour and more evident consistence), we may still apply linear extraction with great benefit. In such cases it is commonly necessary, after making the corneal incision, to excise a portion of the iris, and then lacerate the capsule with a cystotome, as for extraction; now pass a small scoop or spoon carefully into the wound, press it a little backwards so as to pass behind the lens, and continuing onwards in that position, bring the scoop back filled with

cataractous lens-material, and, pressing slightly on the posterior lip of the wound, bring it out. This manœuvre will have to be repeated, and often more than once, always avoiding roughness—passing through the space created by the iridectomy: not dragging on the eye; and remembering, as you bring out your spoon, not to press its contents forward so as to impact them against the cornea. The clearing of the pupil is to be aided by applying the front of the forefinger to the border of the cornea opposite the curette, and exercising a slight pressure so as to aid the exit of any soft matter which has been displaced but not removed. Linear extraction thus combined with iridectomy (Schüft) is an operation of great value in cases of soft cataract, or gelatinous cataract with hard nucleus; of semifluid cataract with calcareous deposits; of diabetic cataract. Where I excise the iris, I usually prefer the superior incision of the cornea; for the eyelid covers the coloboma, and removes inconvenience. The knives which you see me employ are those of De Graefe and Jäger; except that, as I employ this method for young children, and frequently make but one stage, passing the lanceolar knife straight on through the dilated pupil, and allowing the fluid cataract to escape in the manner I have explained, I have some lances which are at once smaller, and relatively longer and narrower than usual. To facilitate the escape of the fluid cataract, the lance may be well grooved on its anterior face, somewhat like the grooved needle-knife recommended by Walker for the extraction of a soft dislocated lens. For the second class of cases, where the cataract is only more or less soft, and where after the linear incision of the cornea it is necessary to excise the iris and introduce the scoop, I use a modified form of Schüft's scoop.

"Where you propose to perform iridectomy it is undesirable to employ atropine to dilate the pupil. In the many cases where you will be able to omit all excision of the iris, the pupil should be fully dilated. Chloroform may be given with safety, and often with advantage; but it must not be pushed to complete insensibility. Your patient should be properly prepared (as to abstinence from food, &c.), and should not be moved for some time, to avoid vomiting. A carefully adjusted compress also should be applied over the eyelid.

"The main precautions for success in linear extraction are—1st, to select your case properly, by avoiding large hard cataracts; 2nd, to use a comparatively small scoop; 3rd, to avoid bruising the iris (if bruised, always to excise it); 4th, to use so much gentle firmness and persistence as to leave a clear pupil, free from opaque matter, before considering your work at an end.

"Linear extraction gives results which are, in some respects, incomparable. The operation is less grave than flap extraction; it is not liable to some serious primary or secondary accidents, such as considerable escape of the vitreous, collapse of the globe and haemorrhagic effusion. It may be practised with success in cases where you would hesitate to extract by flap, as diabetic cataract. It is by far more perfect in principle, and better in practice, than solution by needles for a number of cases of fluid and semifluid cataracts.

The patient sees after the second or third day. The confinement is less protracted, the period of seclusion in bed less tedious and oppressive, and the bandage may be removed, and moderated light admitted to the shaded eye, from forty-eight to sixty hours after the operation. To some persons the continued darkness necessarily enforced after flap extraction is peculiarly intolerable. But linear extraction may be deservedly studied and developed in its applications, mainly as supplementing, partly as superseding flap extraction. It is rather to be regarded as a valuable improvement in many cases on 'drilling' and 'discission.' It is capable of rendering great service, and I will call your attention to it in all cases in which we may employ it."

ART. 79—*On Extraction of Soft Cataract by Suction.*

By MR. T. PRIDGIN TEALE, Jun., Surgeon to the Leeds General Infirmary.

(*Lancet*, September 24, 1864.)

Details of the Operation.—The operation by suction which Mr. Teale has adopted is founded upon, and is essentially a modification of, "linear extraction."

First stage: *The efficient rupture of the anterior capsule.*—The pupil having been dilated by atropine, and the eyelids fixed by the stop-wire speculum, the anterior capsule of the lens should be very freely torn open by two needles passed through the cornea from opposite sides. In carrying out this step the surgeon should bear in mind that its object is not merely to liberate the cataract, but also to ensure such a tearing up of the anterior capsule that it may curl back from the area of the pupil and be lodged behind the iris. At the same time he ought by all means to avoid injuring the *posterior capsule*: a caution to be especially remembered in cases where the cataract is dwarfed and the anterior capsule tough, or where, in traumatic cataract, the lens has been much reduced in bulk by absorption. If the operator wishes to avoid the use of the two needles, he may rupture the capsule at a later stage by introducing through the corneal opening made for the curette the hook used in extraction of hard cataract. The two needles, however, give more perfect command over this important step in the operation.

Second stage: *The opening in the cornea.*—Having withdrawn one needle, and steadying the eye by means of the other, the operator should next make an opening in the cornea for the admission of the tubular curette of the suction instrument. For this purpose a broad needle has been made by Messrs. Weiss, of such a breadth as to make an opening of the exact size required for the curette. The needle should enter the cornea opposite the margin of the pupil when fully dilated, and, passing somewhat obliquely through the laminae of the cornea, should make a valvular opening, in order, firstly, that it may not be too central and leave a scar in front of the pupil; secondly, that it may not be too near the attached margin of the iris,

and thus favour its prolapse and adhesion to the wound ; and thirdly, that the curette, when introduced, may not rest upon nor bruise the iris.

Third stage : *The removal of the cataract by suction.*—Having carefully introduced the curette (if it hitches in traversing the corneal wound, it may easily be disengaged by being turned edgewise), the surgeon should hold the open end of the tube steadily within the area of the pupil, gently burying it in the opaque material. The suction power may then be applied, and regulated in degree as the opaque matter runs off into the tube. As soon as the pupil is clear, the curette may be *carefully* depressed towards the posterior capsule in order to ascertain whether any opaque matter remains, but it should not on any account be swept before or behind the iris. If the suction be continued after the opaque matter has been removed, the cornea is drawn down over the open end of the curette, and blocks it up, thus preventing the iris from being sucked into the instrument and injured.

If the operation has been efficiently performed, it will be found that the cataract has been completely withdrawn from the eye, through an opening in the cornea no larger than would admit the common curette, without any injury to the iris, without rupture of the posterior capsule, and with such complete division of the anterior capsule that it has disappeared completely behind the iris. It will be also found, I think, in the majority of such cases, that recovery is most speedy, that the operation is followed by little or no irritation of the eye, that the patient on the eighth or tenth day can read No. 1 (Jäger), and that the conditions which usually produce opacity of the capsule have been provided against.

The foregoing rules apply to a simple case of complete soft cataract. They are also applicable, with slight modification, to cases of traumatic cataract of recent occurrence. In these cases, however, it is necessary, in the first place, to be very careful to tear open completely the anterior capsule which may have been previously ruptured in the accident producing the cataract ; and, in the second place, to bear in mind that the posterior capsule may also have been torn through. Should this have occurred, the suction operation will be complicated by the admission into the anterior parts of the eye of the vitreous humour, which would tend to pass through the tube more readily than the denser material of the cataract. When such a defect occurs, it is sometimes possible, by careful management of the curette, to withdraw the opaque lens without at the same time drawing off a serious amount of vitreous humour.

Another complication may arise—namely, partial cataract, in which the nuclear portions of the cataract are opaque, and the cortical portions are healthy, tenacious, and adherent to the capsule. This difficulty must be met in the same way as in “linear extraction” by the preliminary operation of puncturing the anterior capsule, so as to admit the aqueous humour into the structure of the lens, and so to cause its disintegration. It may be possible to withdraw by suction even a partially sound lens without the preliminary disintegration ; but I have not yet attempted to do so, not from any diffi-

culty in drawing such healthy lens through the tubular curette, but because, when the peripheral parts of the lens are transparent and adhere to the capsule, it is hardly possible to ascertain when the capsule has been completely cleansed from the lenticular matter.

Another class of cases presenting difficulties is that in which a soft cataract has become wasted and calcareous, or partly so. In such cases the solid portions will not pass along the tube of the curette.

On former attempts to extract Cataract by Suction, and their failure.—When Mr. Teale first devised the suction-curette, he was under the impression that he had hit upon a new idea, and that the proposal was original. In fact, however, there have been various previous attempts to apply the same principle to the extraction of cataract.

The Persians, ages ago, are said by Avicenna to have sucked out cataracts through a hollow needle. How far they succeeded we have no means of judging.

In 1847, M. Laugier invented his "aiguille à pompe," a hollow needle fixed in a syringe, apparently like that now in use for subcutaneous injection. Its use is discussed by M. Desmarres. The needle having been thrust through the sclerotica, vitreous humour, and posterior capsule, and lodged in the centre of the lens, the suction was applied by means of the syringe in the handle of the instrument. If the cataract were fluid, it was drawn into the instrument; the pupil became clear, and sight was immediately restored. If the cataract were not fluid ("et la cataracte liquide est fort rare"), the vitreous humour was drawn out, the cataract was left *in situ*, and the eye collapsed. This misadventure was followed by internal inflammation of the eye, and in consequence the operation was condemned. "En résumé, l'opération de la cataracte par succion est abandonnée." Failure in this operation was to be expected from using the needle of the syringe as the piercing instrument, and from traversing the sclerotica and rupturing the posterior capsule, which ought to have been preserved as the barrier between the cataract and vitreous humour.

Again, M. Blanchet brought forward another method of extracting cataract by suction. Having dilated the pupil, he made an opening in the cornea with a broad needle, through which he introduced a small tube with a flageolet-like mouth attached to an Anell's syringe. With this blunt tube he pierced the capsule of the lens; and if the cataract proved soft, he pumped it out through the tube by working the piston of the syringe. The main defects of this plan of M. Blanchet were—first, the attempt to puncture the anterior capsule with a blunt instrument, thereby using unnecessary force in reaching the cataract; and secondly, the imperfect opening of the anterior capsule, whereby the capsule remained in the area of the pupil, and, becoming opaque, rendered a secondary operation necessary.

On Suction Instruments.—The original suction instrument which Messrs. Weiss made for Mr. Teale consisted simply of a tubular curette fixed in a handle, to which a small india-rubber tube with a mouthpiece is attached. The flexible tube is of such a length as to

reach from the mouth of the operator to the curette when held in the eye.

Shortly afterwards Messrs. Weiss made, at the suggestion of Mr. Bowman, a modification of this instrument, in which the suction power is applied by an ingenious mechanism in the handle, so that the hand which holds the curette controls the suction. Mr. Bowman also inserted a piece of glass tube between the curette and the handle, to enable the operator to watch the result of the suction.

A third instrument has been suggested and made for Mr. Teale by Messrs. Weiss, which is simply a light glass tube with the tubular curette fixed at one end, and the flexible tube with a mouthpiece at the other end.

A fourth instrument has been made for Dr. Bader, of Guy's Hospital, by Khrone, of Whitechapel, and is thus described by Mr. Lawson :—“The suction power is a small, hollow india-rubber ball, placed at the extremity of a tube which terminates in a glass tubular curette. Pressure is made on the ball with the hand to expel the air from the tube, and its readmission is regulated by a well-contrived stop apparatus placed close to the curette. After the air from the ball has been expelled and its readmission prevented by closing the stop, the curette is introduced into the eye, and the amount of suction is regulated by a little trigger connected with the stop apparatus within.”

Having used the first three forms of instrument, Mr. Teale has found them to do their work perfectly; and he has no doubt that Dr. Bader's is at least equal to them. On the whole, perhaps from having used it more frequently, he prefers the original curette (with the addition of the glass tube), as the suction is more immediately at command when applied by the mouth, and the instrument can be guided with greater delicacy when the hand is not fettered by applying the suction power.

Cases of Extraction by Suction.—Mr. Teale selects the following from several in which he has extracted cataract by suction :—

CASE 1.—G. C. D— ; cataract produced by grains of powder shot into the eye eighteen months previously.

Dec. 18th, 1863.—Extraction by suction.

21st (fourth day).—Reads No. 10 (Jäger).

24th (seventh day).—Reads No. 4.

Jan. 13th, 1864.—Reads No. 1.

CASE 2.—G. A—, aged ten ; cataract caused by a blow three weeks previously.

Jan. 1, 1864.—Extraction by suction ; some opaque capsule remaining in the area of the pupil.

18th.—Reads No. 20.

28th.—Reads No. 4. The opaque capsule has spontaneously opened out, but there is still a slight film, preventing perfect vision.

CASE 3.—Feb. 1864 : M. E. T—, aged seven ; an unhealthy child. Extraction by suction, followed by iritis and partially closed pupil. This is the only case of suction in which I have seen serious inflammation.

CASE 4.—Mrs. A— ; partial cataract, probably caused by a blow three years previously.

May 17th, 1864.—Capsule freely punctured.

30th.—Extraction by suction. Reads No. 20.
 June 6th (eighth day).—With +2½ reads No. 2.
 8th (tenth day).—With +2 reads No. 1.
CASE 5.—E. S—, aged seven months; complete spontaneous cataract in both eyes.

May 17th, 1864.—Extraction of both cataracts by suction.
 22nd (sixth day).—Returned home into the country, with pupils clear, central, and active, and with the eyes free from irritation.

The following cases were operated on by Mr. Samuel Hey in the Leeds General Infirmary :—

CASE 6.—M. S—, aged twenty-eight.
 Right eye—complete spontaneous cataract of four years' duration :
 June 2nd, 1864.—Extraction by suction.
 6th (fifth day).—Reads No. 16.
 9th (eighth day).—Reads No. 1.
 Left eye—spontaneous cataract of two years' duration :
 June 16th.—Extraction by suction. In this eye the capsule was tough, and the lens not completely opaque, so that some transparent portions were left behind.
 19th.—Pupil filled with opaque material. The eye painful and congested.
 July 14th.—Left the hospital. Pupil clear; no inflammation.
 Aug. 17th.—With +2½ reads No. 4.
CASE 7.—B. F—, aged sixteen; suffering from diabetes. Complete spontaneous cataract in both eyes, of two months' duration.
 Right eye :
 June 2nd. Extraction by suction.
 9th.—Pupil perfect; free from opaque matter. No congestion of the eye.
 Left eye :
 June 16th.—Extraction by suction.
 23rd.—Pupil perfect; free from opaque matter. No congestion of the eye.
 As she was not very intelligent, and could not read, we were unable to test her vision by type; the vision, however, of both eyes was excellent.

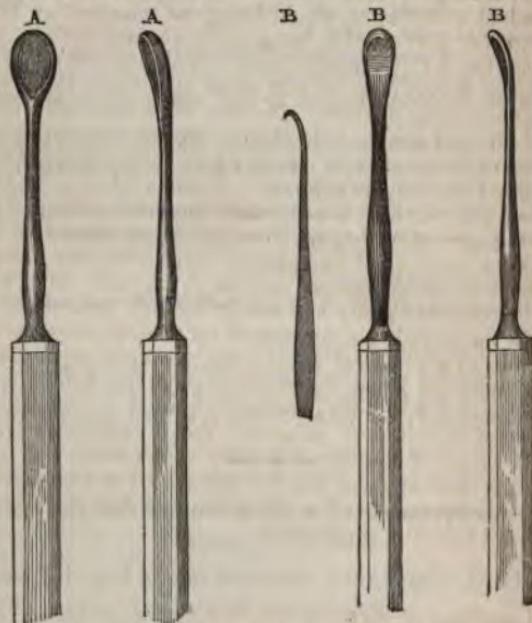
ART. 80.—Description of a New Instrument for the Extraction of Cataract.

By Mr. G. CRITCHETT, Surgeon to the Royal London Ophthalmic Hospital.
(Lancet, June 11, 1864.)

Mr. Critchett writes :—"I propose, for reasons I will presently explain, to call this little instrument the Vectis Spoon."

"Some time ago an important modification in the usual method of extracting a cataract was proposed by Schüft of Berlin, which I must briefly describe :—The eye having been exposed by an ordinary wire speculum and fixed, a keratome, or spear-shaped knife, is introduced near the margin of the cornea and made to traverse the anterior chamber so as to form a slightly curved slit or opening, about three and a half lines in extent; a portion of iris is then drawn through the corneal opening and cut off. The capsule of the lens is freely opened, and then the most important and critical part of the

proceeding commences—viz., the removal of the lens. This he proposed to accomplish by a peculiar form of spoon with perpendicular dished edges (see Fig. A). This spoon is carefully introduced behind the lens and then withdrawn, so as to bring away a spoonful of lenticular matter; and this proceeding is repeated until, as far as possible, the entire cataract is removed. If this can be accomplished without the loss of vitreous humour, without bruising the iris, and without leaving fragments of the opaque lens in the eye, and probably concealed behind the iris, I am strongly of opinion that the chances of success are decidedly greater, and the anxieties connected with the after-treatment of the case far less, than when the old method of extraction by means of a large semicircular flap is employed. Some considerable experience of this operation, dating



back from its first proposal by Schüft, whilst it has convinced me of its great value in many cases, has also impressed upon me several important objections and sources of failure, most of which are removed by the form of spoon that I wish to substitute. In the first place, Schüft's method is inapplicable to the hard amber senile cataract: it is impossible to pass the thick spoon behind the posterior surface of the hard cataract without rupturing the hyaloid membrane; and even if this were accomplished, it would be scarcely possible to remove a bulky hard lens, together with a thick spoon occupying considerable additional space, through a slit in the cornea. In the second place, even in cataracts of medium density, there is often considerable difficulty in passing this form of spoon behind the

lens; it is apt to become early entangled in the substance of the cataract before it has passed behind its posterior surface, to break it into fragments, and to *push portions into the space behind the iris*, where they remain concealed until the aqueous humour gradually floats them into the pupil, where they excite irritation, and either retard the progress of the case, or even seriously compromise the eye. Such being some of the more serious objections to this form of spoon, I was led to adopt what I consider to be an important and valuable modification. The indications were as follows:—An instrument that by its form would glide between the posterior surface of the cataract and the hyaloid membrane without resistance and without becoming entangled in the substance of the former, that would adapt itself accurately to the posterior surface of the lens, and that on its gradual withdrawal, would bring the cataract, or at any rate the firm nuclear portion of it, away unbroken and without taking up more space than a thin metallic layer. A glance at the peculiar form of the Vectis Spoon (Fig. b) will show that these three important indications are accomplished. The thin edge facilitates greatly the gliding of the instrument behind the cataract, as it offers no resistance whatever, but makes its way as a boat moves through the water; having passed behind the cataract it forms a concave bed, in which it may rest, and to which it accurately moulds itself; and when the withdrawing movement begins, the edge, which offered no resistance as long as the instrument was passing behind the lens, now slightly imbeds itself in its substance, and earns its name of "Vectis" by drawing the cataract easily and surely through the corneal opening. I have now employed this instrument on several occasions for some months, and, as a practical man, I am strongly impressed with its value and with its great superiority over the Schüft spoon. It enables us to deal with all varieties of cataract, however dense (I do not, of course, include soft cataracts), and I venture to predict that when this mode of operating is fully understood, the safety and facility in its performance will be recognised as so much increased by the Vectis Spoon, that the old method of flap extraction will be nearly, if not entirely, superseded.

AET. 81.—*Case of Extreme Squint cured, without Operation, by the Use of Prisms; with Clinical Remarks.*

By Mr. ERNEST HART, Ophthalmic Surgeon to St. Mary's Hospital.

(*Lancet*, August 1, 1864.)

The following case is one of interest, as belonging to a class of instances of squint in which, by a careful distinction of causes, the deformity may be treated upon a scientific basis, and radically cured by a simple adaptation of optical means without any operation. Mr. Hart observes in reference to it, that a careful study of the origin and nature of various forms of squint shows that the treatment must, to do justice to the patient, be almost as much optical as

purely surgical ; and there is a large proportion of cases in which merely optical means succeed perfectly, either in averting the formation of a permanent squint, in curing it when the proper glass is applied early, or in preventing relapse after operation.

J. C., aged twenty-seven, a sailor, of good general health, and had always possessed excellent sight. In May, 1863, he had a severe attack of rheumatic fever; various joints were attacked in succession, and he lay for nearly two months helpless in bed. Towards the end of the attack the eyes became affected : the light became painful to them ; the eyeballs he describes as having become blood red, and acutely painful, the pain being severe over the brow, and darting through the head from the frontal region. The face was blistered by the abundant and acrid lachrymation. Thus there seems to have been an acute rheumatic ophthalmitis. He recovered well, and is not aware that there were any immediate traces left of the disease. He went a voyage to the Cape, and came home in satisfactory health ; but in the subsequent voyage, in December, 1863, he became aware of a certain dimness and confusion in looking at objects, a difficulty in measuring distances, and in defining the outlines of things and persons before him. This came on, he thinks, quite suddenly ; and in the course of the day he found that he had a decided convergent squint of the right eye, and that he had lost control over the movements of that eye. He consulted Mr. Hart in the middle of January last.

Mr. Hart found then a convergent squint, monolateral, of the right eye, permanent, and due to paralysis of the external rectus. The visual powers of the two eyes, tested separately, were very nearly equal. There was no dilatation or semi-dilatation of the right pupil ; no drooping of the lid. The latitude of accommodation was equal to that of the left eye. The patient suffered greatly by the constant confusion arising from the double set of images which the incongruous eyes received. He was frequently unable to distinguish between the true image and its ghost, and, besides the giddiness and confusion thus occasioned, felt himself in danger in walking the streets.

Mr. Hart called the attention of the class especially to the case as one in which, from the positive and negative data above mentioned, the cause of the squint might be accurately determined. He referred it to insufficiency (*paresis*) of the right external rectus. The normal action of the other muscles of the eye and eyelid and of the iris, excluded cerebral causes ; while the absence of far-sightedness or short-sightedness in any marked degree, and the healthy appearance of the internal fundus of the eye revealed by the ophthalmoscope, excluded dioptric or retinal disorder from the etiology of the case. The cause being thus determined, it remained to decide on the remedy. Mr. Hart pointed out that in this case the cause of the double images which occasioned so much distress was that incongruous portions of the two retinae were, by virtue of the displacement of the axis of the strabismic eye, impressed with the respective images of each object seen. This might be remedied, then, by the use of a prismatically ground spectacle-glass, with the base turned in

the direction opposite to that of the squint; for such a glass has the power of causing a deviation of rays of light incident on its base, which may thus be used to deflect the rays proceeding from objects looked at, so that they may, in the case of the squinting eye, be made to fall upon a part of the retina congruous to that which receives them in the normal eye, and thus binocular vision be restored. In fact, a prism may be used thus to fuse and destroy double images, just as, conversely, with healthy eyes, it is sometimes used in sport to produce them. Taking a series of prismatic glasses ground to scale, Mr. Hart then essayed them with this patient before the class. A glass ground to an angle of twelve degrees placed before the deviating eye so altered the direction of the images which it received as to fuse the double images into one. This, however, would only have removed the visual inconvenience without curing the squint. By now selecting a glass of ten degrees the images could be nearly fused, but not quite; and then the horror of double images, which is instinctive, caused an involuntary effort of the semi-paralytic and enfeebled rectus externus, which just succeeded in drawing the eye so far further outward as to compensate for the diminished angle of the prism, and fuse the double image. This, then, was the glass selected for the patient to wear; for by the aid of this glass the enfeebled muscle was, as it were, gymnastically exercised and strengthened. The fusion of images was not effected without an effort somewhat painful, and which could not long be sustained. Mr. Hart, therefore, directed the patient to employ the glass at intervals only during the day, gradually lengthening the period of exercise. In the course of seven days he had made considerable progress, could wear the glasses much longer at a time, and fuse the images more thoroughly and with less effort. He was then very anxious to rejoin his ship. Mr. Hart, therefore, furnished him with glasses of eight, six, and four degrees respectively; so that as the muscle became stronger a correspondingly greater effort might be required of it; the glasses fulfilling a true gymnastic as well as optical function, and acting just as dumb-bells of graduated weight, with appropriate exercise, may be made to act, in dealing with enfeebled muscles in other parts of the body. The patient was desired to communicate the result, or to show himself on his return. Recently the man presented himself quite cured. This cure was effected in the course of eight weeks. And in June he wrote to say that his eyes are now as good as ever, that the movements of the two are synchronous, and vision normal.

ART. 82.—*Utility of Phosphorus in the Treatment of Scrofulous Inflammations and Nervous Affections of the Eyes.*

By M. TAVIGNOT.

(*Gaz. des Hôp.*, No. 49, 1863; *Schmidt's Jahrb.*, No. 5, 1864.)

Tavignot reports that he has had large experience of this treatment in the most various eye-affections. The following is the pre-

scription :—Phosphorus, 10 centigrms.; sweet-almond oil, 8 grms.; soap, 10 grms. 40 centigrms.; pulv. altheæ, 18 grms.; subcarb. of iron, 10 grms. Make one hundred pills, covering them with a layer of gelatine to prevent the evaporation of the phosphorus. One to be taken morning and evening, on an empty stomach; in rare cases, as many as five pills are administered daily. Tavignot ascribes to the phosphorus a special influence on sanguification; he gives it also in cases of retarded menstruation, and in all scrofulous conditions.

ART. 83.—*On Luminous Eyes.*

By Dr. K. STELLWAG VON CARION.

(*Wien. Med. Wochenschr.*, 1864, pp. 145, 161, 177, and
Ophth. Rev., July, 1864.)

The fundus oculi reflects light both in a regular and irregular manner. Of the former we have an example in the peculiar shining of the eye in dogs or cats; of the latter, in the light reflected back to the observer with the ophthalmoscope. After describing the structure of the tapetum, to which the lustre of an animal's eye is principally due, the author passes to the similar phenomenon in the human eye. So far as he is aware, Fermin, who saw it in 1796 in an albino, was the first to mention it, and J. Beer the first to pay any particular attention to it. The latter considers it to be immediately owing to a pathological disappearance of the choroidal pigment (cats'-eye amaurosis). This appearance soon came to be considered a symptom of medullary cancer of the retina. The assertion is quite correct, that such a disease at an early period frequently produces exactly similar reflections to those described by Beer; for cancer is wont to occur as an infiltration, which distends a greater or less part of the retina into a thick layer. Now, as the latter covers the pigment-layer of the choroid, and, owing to its little transparency and light colour, diffuses and reflects a large amount of the incident light, the fundus becomes illuminated and appears red, whitish-red, or whitish, according to the vascularity of the new formation. Moreover, as the tumour grows, it passes more and more within the focus of the media, and therefore is seen in an erect and magnified image—the vessels, little elevations, &c., becoming distinctly visible. Finally, light is regularly reflected by the surface of the tumour; the eye becomes luminous when toward dusk it is placed in a certain position with reference to the light and to the observer, the morbid product taking the place of the tapetum in an animal's eye.

The observations of Travers, Ammon, and others, show that sometimes after this symptom has been observed the eyeball gradually became softer and atrophic. This was at first explained by the supposition that the supposed cancer had undergone a retrograde metamorphosis—a supposition which was, however, proved to be incorrect by Chelius. Now-a-days, it is known that in such cases the tumours are of inflammatory origin, that they are formed of

connective tissue, and that they usually proceed from the choroid, although, in exceptional instances, they may be primarily developed in the retina. Such tumours constantly end by suppuration or atrophy ; they, of course, destroy the eye, but do not, like cancer, destroy the patient. They now pass under the title of sarcomatous tumours.

It must not be imagined that the lustre is a constant symptom either of cancer or sarcoma ; its occurrence depends on certain conditions. In the first place, the surface of the tumour must be of a light colour, and not too rough. In sarcomatous tumours of the choroid, which contain a large quantity of pigment, or which are covered by the unaltered pigment-layer of the choroid, and in intra-ocular melanosis, no lustre is observed. Stellwag also believes that there is no lustre in cases of sarcoma or cancer where the surface projecting into the vitreous body is very irregular and vascular. For choroidal tumours to regularly reflect light there is another condition : the retina must have either been ruptured, or be stretched as a tense and even membrane. This phenomenon is not found in cases of separation of the retina from the surface of the tumour by serous fluid, or, indeed, in the ordinary cases of retinal separation, for the membrane is too transparent to act efficiently as a mirror, and yet too opaque to allow sufficient light to pass to the surface of any subjacent tumour.

This phenomenon seems to be specially promoted by a light-coloured choroidal tumour pressing the retina forwards, so as to apply it exactly to the posterior surface of the lens. The author has scarcely ever seen it so beautifully as in an albino, two and a half years old, whose right eye was affected in the manner just mentioned. Even in a well-lighted room there was a very marked mother-of pearl lustre, but in semi-darkness the eye emitted truly brilliant bluish and yellowish reflections, whilst the left eye sparkled of a ruby-red. In the right eye the cornea was perfectly transparent—the iris reduced to an extremely narrow, brownish-black, immovable border ; the pellucid lens was in contact with the cornea, and its posterior surface was completely covered by the retina. The latter was very opaque, and of a greyish-yellow colour ; here and there were bluish-white stripes, and spots containing masses of cholesterine. A light-coloured tumour situated behind it appeared as if covered by a thick veil. The eye was already atrophying.

A separated retina may cause such a peculiar reflection under less favourable circumstances, provided it is thickened and transformed into a dense, tendinous mass.

There has been some doubt as to the nature of the affection described by Beer as "cats'-eye amaurosis." Himly refers it to absorption of the choroidal pigment, and supports his view by the light colour of the fundus oculi and the lustre observed in albinos. It is true that some observers have not noticed the latter appearance, probably owing to their neglect of the conditions under which it can be seen. If an albino is placed in a partially-darkened room opposite a moderately-distant window or lamp, a properly-placed observer will scarcely ever fail to perceive the lustre—only, indeed,

when the pupil cannot dilate. The author considers rapidly advancing myopia to have been the essential lesion in Beer's cases; the extreme atrophy of the choroid and consequent exposure of the sclerotic very much conduce to the production of the lustre; and the author can, from his own experience, affirm that it may always be seen in the most brilliant manner in such cases, provided the external conditions of the phenomenon are fulfilled. Even if there is only a posterior staphyloma, or if the surrounding choroid is but little atrophied, the phenomenon can always be perceived, provided the pupil is well dilated and the eye properly directed with reference to the lamp.

As this lustre could be perceived where the staphyloma was but small, it appeared probable that it might also be seen in cases of fibrous degeneration of the optic disc. This supposition was confirmed by the examination of a number of such cases; indeed, the lustre was very marked. The ophthalmoscope showed in the mass of these cases that the choroid was quite normal, the atrophy of the optic nerve being a consequence of simple neuritis optica, or depending on cerebral amaurosis. In one case there was an old and very characteristic glaucomatous excavation.

The same lustre may be seen in normal eyes: it is very difficult to render it apparent so long as the pupil is contracted, but if full dilatation is effected by the use of atropine, there is not much trouble. For the same reason, it is a constant symptom where there is a congenital absence of the iris.

From what has been said, there can be no great difficulty in determining what is the part that takes the place of a mirror. If the choroidal pigment is entire, the room must be darkened, the lamp placed at a distance of several feet from the eye to be examined, and the observer must place a screen between himself and the flame, so that he may look almost exactly in the same direction; at the same time, the eye observed must be directed somewhat inwards. This is the only position in which the lustre is observed. The experiment is much facilitated by full dilatation of the pupil. It is hence evident that there is in the normally-pigmented fundus no considerable surface which can act as a speculum; the opinion that the optic disc is the reflector appears to be confirmed by the fact already mentioned, that the lustre is more readily observed when the nerve has undergone fibrous degeneration.

Where, from any morbid process, the choroidal pigment has been extensively destroyed, the exposed sclerotic takes the place of the tapetum. For exactly analogous reasons, the lustre is very marked, and very readily perceived in albinos.

Retinal cancers, light-coloured choroidal tumours, fibrous degenerations of the separated retina, very closely resemble the tapetum in their physical qualities.

ART. 84.—*A Note on Reflex Disease of the Eye, of Traumatic Origin.*

By Mr. J. VOSE SOLOMON, Surgeon to the Birmingham and Midland Eye Hospital.

(*Dublin Quarterly Journal of Medical Science*, February, 1863.)

The following case goes to show that the mechanical irritation of an orbit from which the globe has been removed is capable of producing symptoms of reflex disorder in the previously healthy eye of the opposite side.

CASE.—W. V.—, a healthy man, aged thirty-six, by trade an iron-moulder, received a severe burn of his *left* eye, from the spouting of some molten iron, on the 1st of July, 1857. He obtained prompt and careful treatment, yet the vision was lost, and a connecting band formed between the globe and its lower lid. Early in the following September the right eye suffered sympathetically. It was painful, and there was a sensation of external compression. The vision, which was best in bright light, became, at the same time, disturbed by what appeared to be a cobweb, which constantly floated before the eye. The pain and dulness of sight increased so much that, on the 17th of March of the following year, 1858, the patient requested me to remove the disorganized globe, which was accordingly done by enucleation, in the usual way. The optic nerve was divided close to the globe. In three hours after the operation there was a marked mitigation of the severe cutting pains which had affected the right eye during the last three or four months, and the cobweb (*musca*) appeared to be much smaller. In the course of a short time the band which connected the lower lid to the globe was regenerated, and presented an obstacle to the introduction of an artificial eye. It was therefore divided, and a glass mask inserted in the orbit. The mask excited a good deal of local pain and conjunctival inflammation, which, in its turn reproduced sympathetic ocular disorder of the same degree and kind as had existed before the excision was performed. On the withdrawal of the mask, and subsidence of the conjunctival irritation, the left eye assumed its former condition of usefulness. At the end of a fortnight the mask was once more used, when the sympathetic symptoms reappeared—flashes in the dark (*photopsia*) being superadded. The patient has now for more than four years been engaged in his trade, as an iron moulder, taking the precaution of ceasing work whenever the eye becomes dim or affected with flashes. The sight is generally clear, and he is capable of reading small print without fatigue.

English authors on ophthalmology have hitherto attributed the occurrence of sympathetic traumatic ophthalmia to the commissural arrangement of the optic nerve fibres.

The phenomena presented by the preceding case do not appear to afford a *locus standi* for such theory, inasmuch as the optic nerve on the side of irritation had been severed; but they may be satisfactorily explained on the supposition that the irritation from the eyeless orbital cavity was conveyed by the ophthalmic division of the fifth pair of nerves to the centre, and thence reflected to the opposite side. This view derives support from examples of reflex traumatic disorder wherein the symptoms are purely those of hyperæsthesia of

the fifth, and which yield, almost *instantaneously*, on removal of the original source of irritation.

Careful observation of a large number of instances of reflex ophthalmia—a disease of great frequency in Birmingham, and the neighbouring mining districts—has led Mr. Solomon to the following, among other conclusions, namely:—that the primary traumatic irritation is conveyed to the sound eye through branches of the fifth nerve; that soon afterwards, and sometimes simultaneously, the vasomotor nerves take on disordered action, and, as a consequence, intraocular congestions and their results occur; that if the disease be not arrested, the optic nerve fibres become inflamed, or affected with such other changes as induce atrophy, and which lead to similar degeneration of the opposite nerve, attended by an amaurosis which is incurable. In this stage of the complaint the commissural arrangement of the optic nerve fibres is probably the channel through which the disorder is conveyed.

When the state of the eye admits of its fundus being examined by the ophthalmoscope, in the early stage of reflex ophthalmia, the most constant appearance is congestion of the vessels of the retina, and more especially of the veins; but where there is an advanced amaurosis the usual signs of atrophy of the optic nerve entrance, which are sufficiently familiar to render description in this place needless, are revealed.

ART. 85.—*Embolism of the Arteria Centralis Retinæ.*

By M. O. JUST, Jun.

(*Klin. Monatsbl. f. Augenh.* i. 265; and *Ophth. Rev.*, July, 1864.)

CASE.—A pale-faced man, aged thirty-six, who had often suffered from shortness of breath and palpitations, suddenly perceived, on the 11th April, 1862, dark wheels before his right eye, followed in five minutes by total blindness. Three days later he presented the following symptoms:—Eye perfectly amaurotic, even in respect to concentrated sunlight. The optic disc (inverted image) of a bluish-white, its edge at many parts indistinct; the central vessels resemble delicate threads; at the nasal side there is a dark-red circumscribed spot within the disc; the surrounding retina is faintly clouded, so that the choroidal vessels are less distinct; at the macula lutea there is a smoky cloudiness: only scattered portions of vessels can be seen. On examining the erect image, the red patch is seen to be formed by an extravasation. The fundus of the left eye is normal. Pulse 72; heart a little enlarged; in the aorta there is a slight diastolic murmur. Ten days later he had again recovered some perception of light. There could not be a doubt about the diagnosis; for as the blindness had occurred within a few minutes and without any injury, as the arteries were found to be extremely contracted, and as, besides, there was a little insufficiency of the aortic valves, only an embolism of the central retinal artery could be admitted as the cause of the amaurosis.

The author is aware of Schneller's case, which shows that vision may be almost perfectly restored. The most important indication is to lower the action of the heart by rest, light food, and digitalis; blood-letting was entirely rejected. Ten days later, when some sensibility had returned, the

author remembered that Oppolzer had used iodide of potassium with excellent effect in apoplexy ; he determined, accordingly, to administer half a drachm daily. The effect seemed at first very good, for in seven days the patient could recognise movements of the hand in the outer part of the field of vision, and in fourteen days large objects situated in the line of the optic axis. Eight weeks after his first examination he could count fingers, though not with certainty. On the other hand, the symptoms of heart disease were more marked ; the pulse irregular, 104 ; the aortic bruit much louder. The ophthalmoscopic appearances were much the same, except that the disc was whiter and more shining, the retina more clouded.

According to an account which lately reached the author, almost all perception of light has again been lost, probably from atrophy of the retina.

ART. 86.—*Foreign Bodies in the Iris.*

By M. F. HORNER.

(*Klin. Monatsbl. f. Augenh.* i. 395 ; and *Ophth. Rev.*, July, 1864.)

The best plan of treating the cases in which small particles of metal are implanted in the iris is, according to M. Horner, by excision ; any attempt to separately seize the fragment is certain to fail.

CASE 1.—Emanuel Schneider, admitted on the 29th April with a little piece of metal fixed in the iris near the lower border of the pupil ; it appeared as a yellowish tubercle, surrounded by a reddish areola ; the corneal wound was entirely closed ; there was no opacity of the lens. On the 30th iridectomy was performed, the little piece of metal and the surrounding iris being removed at the same time. Eight days after the operation the eye was without a trace of redness, so that he was again fit for work.

CASE 2.—Rudolph Griesser, aged fifty-two, was struck by a piece of wood on the right eye in the middle of April. The sub-conjunctival vessels were slightly injected ; there was a wound near the outer margin of the cornea, and a brownish mark, half a line long, corresponded to it in the iris ; the aqueous humour was clouded ; he could scarcely read No. 3, Jäger. On the 17th May he became an in-patient. On the 18th, after the pupil had been dilated by atropine, a little brownish body was perceived on the lower and inner part of the iris : it was clear that this was the body which had been previously perceived as a brownish mark in the iris ; the lens was perfectly clear. Again both the iris and foreign body were excised ; the patient was fit to be discharged on the 22nd May.

CASE 3.—H. Hugentobler received an injury of the left eye on the 17th July. On the 27th he was examined ; the wound of the cornea was opposite the outer and lower margin of the pupil, $\frac{1}{4}$ " broad, perfectly closed ; the pupil forms a longish oval, drawn to a point outwards and downwards ; a little collection of pus, which is connected with the corneal wound by a small greyish thread, is observed on the iris, close to the ciliary ligament ; a darker yellow spot was discovered in the abscess, evidently from the foreign body, which was imagined to be of wood. The great injection of the sub-conjunctival vessels, the suppuration, the certainty of the position of the foreign body, were sufficiently cogent reasons for its immediate removal. On the 28th an incision was made through the sclerotic into the anterior chamber ; the iris could not, however, be seized at the side of the foreign body ; attempts to take hold of the latter were equally unsuccessful, the

forceps invariably slipping. The wound was therefore enlarged, and Daviel's spoon slipped under the foreign body, which was now removed without difficulty ; it proved to be a fragment of metal $2\frac{1}{4}$ mm. long and 1 mm. broad. No accident followed ; the patient was discharged on the 2nd August.

CASE 4.—Josef Huber, aged thirty-two, had his left eye injured on the 17th June. According to the account of his medical man, there was near the upper and inner edge of the cornea a wound through which the iris had prolapsed ; there was some blood in the anterior chamber ; a glittering fragment of metal could be seen on the iris. He was treated with antiphlogistics and mydriatics ; the blood was absorbed ; the inflammation continued moderate, yet towards the end of the month considerable exudation from the iris took place, and the prolapsus became much larger. He therefore came under Dr. Horner on the 1st July. The prolapsus, about a square line in size, was at once excised. In the pupillary space there was some brownish exudation ; a rusty-looking mass was seen on the iris, at the bottom of the anterior chamber ; the eye was much injected, and very irritable. A free incision was made through the cornea, but though repeated attempts were made, the iris could not be seized ; the foreign body could not now be seen ; the wound was freely dilated, some iris was removed, the capsule of the lens was opened with the cystotome, and the lens extracted by the use of Schuft's spoon ; the foreign body escaped at the same time ; it was only $1\frac{1}{2}$ mm. long and $\frac{1}{2}$ mm. broad. On the 19th July the patient returned home ; the pupil was still somewhat occupied by exudation, but the iris had recovered its normal appearance, and the eye was free from pain. By a letter of the 19th of August, the author has learnt that the sight is daily improving, and that the patient is at his ordinary work.

The author also relates a case in which a fragment of a guncap, half a square line in size, became encysted in the iris.

ART. 87.—Case of Injury to the Eye, resulting in Total Loss of Iris and Lens, with a Permanent Fistula of the Sclerotica, in which nearly Perfect Vision was recovered.

By Mr. JOHN WILLIAMS, late Surgeon to the Cork Eye Infirmary.

(*Dublin Quarterly Journal of Medical Science, August, 1864.*)

CASE.—Michael Haines, aged forty-six, the subject of this case, was under my care at the Eye Infirmary, for pannus, in the year 1859. I lost sight of him until the 22nd May, 1863, when he came to my residence for advice for "weakness of sight" in the right eye, the result of an injury ; and for a speck on the left cornea, which, as it left the upper third of the pupil uncovered, materially interfered with, but did not completely hinder sight in that eye. On examination, which was rather cursorily made at first, the following appearances were presented by the right eye :—

The eye-ball was misshapen, but was full, and elastic to the touch ; the cornea, which was very irregular in outline, was clear and brilliant, save where it presented two whitish lines, which encroached on it as far as the centre, but which did not appear to intercept vision in the least. I must here remark that comparison with the left eye was very imperfect, as the latter presented—besides other traces of old inflammation—a leucoma on the cornea ; all appeared black behind the cornea, no iris being visible ; the

colour of the left iris was blue, and contrasted much with the black appearance behind the right cornea. At the inferior part of the eye, towards the inner angle, and about three lines from the circumference of the cornea, was a fistulous opening in the sclerotic coat, which communicated with the interior of the globe. Through this fistula a piece of vitreous humour, about the size of a pin's head, protruded. When I made pressure with my probe on this globe of transparent humour, it receded into the depths of the globe, but reappeared when I pressed on the latter. This fistulous opening formed the apex of a conical elevation of the sclerotic, from which three or four dusky white cicatrices radiated—two, as before alluded to, through the cornea, and the other, inwardly and superiorly, through the sclerotic. The sclerotic itself, or white of the eye, was discoloured with dark blue and purplish spots, and had a few tortuous blood-vessels coursing over it. There was external strabismus of that eye.

Having subjected the patient to a more careful examination with the ophthalmoscope, I could not find a trace of either the iris or the lens. A lighted candle held before the eye, and which did not appear to dazzle him in the least, caused but one erect image, showing also the absence of the lens. The orange field, presented by the back of the eye-ball and the retinal vessels, was clouded. I did not feel justified in renewing this ophthalmoscopic examination, however great my anxiety about the case may have been—as, for some days after it, he complained of unpleasant effects. It now appeared evident that the fistula, through which the vitreous humour protruded, marked the site of a rent in the sclerotic, through which the iris and lens had been bodily ejected. This rent or rupture was also evidenced by the whitish cicatrix which surrounded the fistula, and which radiated towards the inner angle of the eye, and also towards the cornea.

Notwithstanding the amount of injury sustained by the eye-ball, with the loss of such important structures, the sight of that eye is excellent, as the patient can see, *without the aid of a lens*, a single hair, pin, or other small object; and what is also most remarkable, he retains full power of adjustment of vision, for he can see the smoke issuing from a chimney 500 yards away: and by instantly directing his sight to a pin held before him, can tell what it is.

Previous history.—He had been head grinder in Perrott's Iron Foundry, where, in the month of December, 1861, he sustained a severe injury in the right eye, from a fall on the handle of an iron plough. He suffered intense pain in the eye from the fall, and bled considerably. High inflammation set in, and the swelling of the eyelids was so great that, for some days, he could not open the eye; when he could do so, he found he was perfectly blind in the eye, and some weeks elapsed before he "could discern day from night." Gradually, however, vision improved, and he was enabled to see his fingers, but "for a long time everything he looked at appeared red," and he was unable to distinguish the natural colour of objects until a great improvement in his sight took place. Absence from Ireland prevented my seeing him for a year after the injury.

October 8, 1863.—On this day I carefully examined him. His vision is steadily improving. He is enabled to engage in the duties of a porter, and in other occupations which do not tax his sight much.

March 22nd, 1864.—Yesterday I took the following note of this remarkable case:—I held a lighted candle before the eye, which, as I before remarked, caused no inconvenience whatever, and found I could illuminate the back of the globe—the orange field not, however, being as distinctly seen as by the light reflected from the mirror of the ophthalmoscope, the blood-

vessels of the retina could not be seen. When all light was excluded from the room this illumination of the globe occurred, but in a less degree. The glare of a cat's eye in the dark, or the phosphorescent appearance of fish, under the same circumstances, closely resembled the condition the eye then presented. On lighting the candle, and getting the patient to stand about six feet from it, he saw, when looking at it *without the glass*, "a great many lights," but when he approached within one foot of it he saw but one flame. I then made him use a No. 5 double convex lens, and *on looking through it*, at any distance from the candle, he saw but one flame. Although he can see a hair or pin with the *unaided eye*, still he finds the lens of much service to him when reading small print. If he looks *directly* at any object, he invariably sees a "round white spot" before it, so that in order to see a thing clearly he directs his vision somewhat above, below, or to either side of it. When he uses the glass, the white spot, under any circumstances, does not appear. When he employs the glass, he holds it from a foot and a half to within a few inches of the eye, according as the object he is looking at is near or remote, large or small. He finds his sight much improved by looking through a perforated card, or piece of blackened tin, which his own ingenuity devised; but, save when he is reading, or is desirous of seeing a small object distinctly, he does not use the glass.

The following are the points connected with this interesting and unique case, to which Mr. Williams would direct special attention:—

1st. The apparent trifling deformity presented by the eye after so serious an injury as rupture of its coats, with escape of lens, and total loss of iris also.

2nd. The wonderful amount of vision enjoyed by the patient *without the aid of a glass*, and the perfect power of adaptation or adjustment he possesses.

3rd. The fact that he is not dazzled by the brightest sun-light, and can use the eye with the same facility as if the iris were present. Although a lighted candle can be held, almost without any inconvenience whatever, immediately before his eye, still the function of the retina must be but little impaired when he is able to see so small an object as a hair without the aid of a glass.

ART. 88.—*On the Treatment of Diseases of the Ear by the Galvanic Current.*

By Dr. BRENNER.

(*Virchow's Arch.* xxiii. 1, 2, p. 197; *Schmidt's Jahrb.* No. 8, 1864.)

Brenner makes statements on this subject which would be highly important if they were accurate, which seems doubtful. He says that on the application of the constant current to a healthy ear, in such a way that one electrode is immersed in some water with which the external auditory meatus is filled, and the other is held in the closed hand, or made to rest on any other indifferent part of the body, sensations of hearing exactly proportionate to the strength of the current are experienced, and also a loud noise at the *closing* of the circuit if the *negative* electrode be in the ear, and a weaker sound at the

opening of the circuit if the positive electrode be in the ear. The loud sound heard in the first case has an echo which continues, gradually diminishing, during the passage of the current. A healthy ear will *always* give these regular reactions, and any weakening of the type of reaction indicates disease of the nerve, according to Brenner: such alterations being either *quantitative* or *qualitative*. In the first case, a stronger current than ordinary is required to produce the usual reaction. The qualitative change may extend even to a reversal of the normal laws of reaction. The appropriate formula of reaction gives at once the electro-pathologic diagnosis, and the prognosis of the nerve disease. The purpose of the galvanic treatment is to restore the normal formula of reaction by "galvanic gymnastics" or "education" of the nerve. By such means the specific energy of the acoustic nerve will be restored or even increased.

[To these views of Brenner, Schwartze strongly objects, and he sums up his own conclusions as follows:—1. Brenner's law of acoustic reaction is by no means universally true in healthy persons. 2. His "normal formula" is contrived for cases of *absolute* deafness, in which alone the possibility of nerve disease (as periphery, trunk, or centre) is to be thought of. 3. The restoration of the normal formula of reaction is not followed by corresponding improvement of hearing.]

ART. 89.—*On the Operation for Cleft Palate.*

By Mr. FERGUSON, Surgeon to King's College Hospital.

(*Lancet*, June 25, 1864.)

The following remarks on this subject occur in one of the Lectures on the Progress of Anatomy and Surgery during the present Century, delivered at the Royal College of Surgeons of England in June, 1864:—

"The early history of the operation for cleft palate sounds like a romance. In 1819 a medical student applied to Roux, then one of the surgical luminaries in Paris, with a defect of this kind. Roux pared the edges of the cleft, and brought them together with stitches. Union followed; the palate became like a normal one, and when the youth appeared amongst his former friends, the change in his voice was such that he could scarcely be recognised as the same person.

"I doubt if this case, although fairly made public by Dr. Stephenson in his inaugural dissertation on Velo-synthesis, when taking his degree as M.D. in the University of Edinburgh in 1820, produced the full effect on the surgical mind that it should, even when further elucidated in the famous essay by Roux, published in 1825. Possibly the rarity of the condition and the difficulties of the operation led to apathy, and down to the period of Roux's death, no one seems to have had any experience on the subject at all equivalent to his. Like others taking their early surgical lessons in the third decade of the present century, I was attracted by the romance

referred to. But I had seen little to absorb special attention. Whilst busy in dissecting-room work, a subject with cleft palate came under my notice. At that time, as even now I suppose, few students took the pains to dissect the palate; but it was my fortune to have this one to luxuriate upon. I made a careful dissection of all the muscular apparatus, and came to the conclusion that I had rarely seen it so highly developed, although the palate and throat were small, being those of an aged female. The whole matter fell aside for years. I had performed the operation on the living body, and had heard of others doing so, without success. The subject in a manner slept on this side of the Atlantic, with the exception of the doings of Roux himself; but about 1840 all Europe, in a surgical sense, rang with the brilliancy of Stromeyer's operations for club-foot and Dieffenbach's for strabismus. Tenotomy and myotomy became the fashionable surgical mania, and I bethought me of my former dissection of the cleft palate. For anything that I knew, it was original. I compared it with the normal condition anatomically and physiologically, and then reflected on what I had seen and heard of surgery as applied to this condition by Roux and others. My zeal was further stimulated by a paper by Dr. Mason Warren of Boston, which told of a larger proportionate success of Dr. Mütter of Philadelphia and himself, than to my knowledge had yet been attained by any others, not even excepting Roux. On additional reflection, I fancied that I had fallen upon new views in anatomy, physiology, and surgery, and my conclusions were embodied in a paper which was submitted to the Medical and Chirurgical Society of London in December, 1844. That paper was honoured with a place in the volume of "Transactions" of the Society for 1845. Its main features went to show how the cleft palate was closed in deglutition by the action of the superior constrictor of the pharynx; how the palato-pharyngei in cleft palate acted differently in this state than in the normal palate, and, instead of closing the opening between the pharynx and the nares, in reality tended to draw the parts asunder—an act which was overbalanced by the vigour of the upper constrictors of the pharynx. Above all, looking to the surgical aspect of the malformation, I gave it as my opinion that the action of the levatores palati probably exercised such an influence on the lateral portions of the palate, after the operation of Roux, as to mar its good intentions. I showed, in as far as one could by reference to the dead and living parts, how the levator muscle on each side had such free and uncontrolled action that, whenever excited, it drew the margin of the cleft outwards and upwards, and so tugged upon the stitches put in by the surgeon that ulceration in their sites and separation of the junction was a most probable result—that, indeed, which had caused the failure of Roux's operation in so many instances.

"The inferences which I drew were, that if the palato-glossus, palato-pharyngéus, and levator palati on each side were divided, the soft flaps would thereafter, for a time, be so relaxed that in all probability the mesial line of adaptation would be so little disturbed that union would take place. The tensor palati I considered would

have little disturbing influence, nor did I put much importance on that of the palato-glossus. My impression was, that the action of the levator palati and palato-pharyngeus, particularly that part in the posterior pillar of the fauces, was likely to prove detrimental; and, in accordance with the somewhat novel and already popular practice of myotomy and tenotomy in other directions, I recommended division of these muscles as an adjunct to the ordinary operation for cleft palate.

"By modern custom, the department of Anatomy associated with the professorship of Surgery in this College has been held of comparatively little account, although both my predecessors have displayed remarkable acquirements in this direction, which they have turned to great account in the field of surgery. As Professor of Human Anatomy, I hope that I may not be out of order in claiming to be the first who solved the problem of how the cleft in the soft palate is closed during deglutition. The drawing influence of muscle has been most recognised; the pushing has been less taken into account, although it is very considerable. Swallowing, the vermicular action of the intestines, and defecation, are notable examples of this force, just as palpable in the estimation of the anatomist as the drawing of the biceps in flexing the arm. Anatomy without physiology would be in a manner senseless, but when joined together they give life and soul to surgery. The influence of the constrictor muscles of the pharynx in the process of deglutition was well known to physiologists; but how, during that process, the gap of the cleft palate was closed in vacant space was an enigma, until I had the good fortune to show that the parts are pushed together by the action of the superior constrictor particularly, so that the gap between the pharynx and the nostril is as completely closed during deglutition as if the velum were entire. Then, for the surgical aspect of the investigation, I showed that by temporarily taking off the influence of such muscles as in common action tended to draw the two portions of the soft palate aside, there was a probability of such entire rest that union in the central line was most likely to take place—certainly, at any rate, more likely than with these muscles in full vigour, irritated, too, as they might be by the wounds, by inflammation, and by the presence of stitches. The almost intolerable distress, the depressing influence, the actual danger, associated with the injunction against swallowing laid down by Roux and others, made the early operations of this kind examples of human endurance which few could follow out to the full extent. Such injunctions had been occasionally disregarded, and Sir Philip Crampton gave some notable examples of this kind. Since I showed, anatomically and physiologically, that during deglutition the parts are actually pushed together, that process is no longer forbidden; and now a fair share of suitable nourishment is freely administered—a matter of great consequence as regards successful issue.

"With a single exception, which shall be nameless on such an occasion as this, I am not aware that any anatomist or surgeon of repute has controverted my views as expressed in the paper referred to. Nor need I do more than advert to the amiable and flattering

device of a continental admirer bringing them all out anew, a few years after, as if they were his own. I have heard of nothing yet to impugn the anatomical explanation which I first gave of how various haphazard incisions might or might not facilitate the approximation and adhesion of the margins of the cleft in the soft palate. The knife for dividing the levator palati was my own device, and it was proposed at the same time, and soon subsequently applied, for the purpose of separating the soft palate from the hard in cases where there seemed a probability of closing a cleft in the hard palate, as well as the soft.

"Those who have devoted attention to cleft palate during the last twenty years must have been surprised at a recent dispute as to priority in separating the soft from the hard palate with a view to close the cleft in the hard. Dr. Mason Warren described this process in 1843; and it was referred to in my original paper. I myself performed it in January, 1845, and have since repeated it on all fitting occasions. The late Mr. Avery devoted special attention to this portion of the palate; and after his death the subject was further ably developed by Mr. Pollock in a paper published in the *Medico-Chirurgical "Transactions"* for 1856. The first idea that I had of this portion of the operation for cleft palate was obtained from Dr. Mason Warren. I deem it but fair to the reputation of that distinguished surgeon to state that I know of no originality before his, and that I look upon all modern claims to such originality as arising either from ignorance or a desire to rob the fair reputation of a name which, in son as in father, will stand for generations among the brightest in surgery.

"Although working at the subject of cleft palate since the date of my first paper, I cannot pretend to add much that is novel to the views which were then expressed. I have little to add, little to detract, from the anatomy and physiology which I ventured to submit as original. I am still as much convinced that the tensor palati has little or no influence on the soft palate—certainly that it has none to counteract the closing of the cleft. The palato-glossus I am of opinion has no practical influence; and, excepting in rare instances, I am convinced from experience that there is no necessity to interfere with the palato-pharyngeus in the posterior pillar. I am equally convinced of the value of dividing the levator palati, for that is the muscle which, by drawing upwards and outwards, separates the edges so as to prevent union or break it up when the stitches are removed.

"There is ample experience to show that union has taken place, despite muscular action to the contrary. The experience of Roux and others has proved that. But I know of no experience equal to my own to prove what I contend for—namely, that by taking off muscular action for a time union can be rendered more certain than by leaving the muscles untouched.

"Since I entertained the views referred to, I have operated on 134 cases, and of these 129 have been successful. In two union failed entirely, and in three it was so partial and imperfect that I place them as unsuccessful. Forty-five of them have been performed in hospital practice. In a considerable number an aperture has been

left in the hard palate, and much benefit has been derived in many such cases from the use of an obturator. Occasionally there is a round aperture left, with such scanty material that it cannot be closed by operation; but in such slits as these, if the roof be wedge-shaped (not horizontal), the soft parts may be separated from the hard, and brought down so as to approximate in the middle, and be held together by stitches until they unite.

" I have never attempted the operation in infancy, and consider the circumstances most favourable at or above puberty; but I have frequently operated successfully at ages between ten and fifteen, where patients have been steady and courageous. I have never operated under chloroform, and whilst I do not deny the possibility of doing so, I am of opinion that, as a rule, it is absolutely requisite to have the patient conscious, so that he may facilitate the steps in a variety of ways.

" In many instances of cleft in the hard palate it is utterly impossible to contend against nature; and even in the soft, the parts are occasionally so scanty that there is literally no material to work upon.

" In as far as I know, the greatest success recorded before my own views were made public was that achieved by Mütter of Philadelphia. In 1843 he had operated successfully in nineteen out of twenty-one cases; and J. Mason Warren of Boston had been successful in thirteen out of fourteen cases. These instances were of both hard and soft palate. What may have been their after-success I cannot say. It has been related of Roux, since his death, that he had operated on 120 cases, and that of these one in every three had failed. I attribute Roux's comparative want of success to the circumstance that the levator palati and back part of the palato-pharyngeus were left untouched; and I consider that Warren's success may have resulted from the free incisions which he made through the palate outside the pillars of the fauces. My own success, if I may so call it, I attribute chiefly to the division of the levator palati, and next to the relaxation which the wound for that division involves. For mere relaxation, the incision of Dieffenbach is probably the most perfect. I know that it has been particularly successful in Mr. Stkey's hands, and in Mr. Pollock's; but, with all deference, I am still disposed, from all I know of the subject, to prefer a free incision above the soft palate, whereby the levator palati may be divided to a certainty. In addition, I look upon this wound as of great service in this respect—the lymph effused upon it acts as a splint, whereby the palate is kept fixed as a board until union in the mesial line is complete.

" As to attitudes in this operation, the patient may sit or lie, as may best suit convenience. Latterly, I have made most use of the recumbent position. I find that the head can be kept best on the same line in this position; and as regards my own views on the anatomy and physiology of the parts concerned, I deem the subject of some importance. For instance, if the patient sits with the head slightly thrown backwards, the palato-pharyngei, when irritated, pull the soft palate downwards towards the epiglottis, so as to leave a space between the palate and the base of the cranium; but if the

head be thrown far backwards the axis of action is altered, and these muscles draw the soft parts upwards, or, in other words, bring the soft palate towards the base of the cranium, and thus add to the difficulties of the surgeon by limiting the space above the soft palate where he has to work with the needles in introducing sutures. Here, as in hare-lip, the surgeon has generally stood before his patient, but I invariably select his right side in preference to all other places.

"The grand practical object of this operation is to improve the voice and articulation. Defective deglutition from this malformation is what attracts the mother's or nurse's attention in early life. The cries of infancy are in nowise peculiar in tone; but when definite articulation commences, or rather should commence, the value of an entire palate is then appreciated. The air and sound, in passing outwards from the larynx, escape in part through the nostrils by the split in the palate. A nasal twang is the result, and articulation as in the normal state of the parts is impossible.

"Immediately after the operation, the modification of the voice can be at once detected. It is customary to keep those operated on from speaking for eight or ten days. It is, however, a needless restriction as regards my operation. In reality, few care, under the circumstances, to speak at all; yet I do not think that it would do harm. In the course of eight or ten days, when the fever or distress following the operation has gone, the tone of the voice is at once perceived to be changed for the better. Improved articulation, however, comes more slowly. Years, many years, are required for distinct articulation when the whole organs are to all appearance in perfection; and after the most successful operation for cleft palate, months and years are required to alter defective sounds. Voice and speech have to be modified anew. With some the changes come slowly and sluggishly; but with others they are so rapid and perfect that in a few years the original defect cannot be detected except by a practised ear."

ART. 90.—*A Description of the First Laryngoscope, as invented and employed by the late Dr. B. G. Babington, in the year 1829.*

By DR. MORELL MACKENZIE.

(*Proceedings of the Royal Medico-Chirurgical Society, April 26, 1864.*)

The object of the present paper is to fix distinctly the epoch of the first employment of the laryngoscope in the investigation of laryngeal disease. In considering this question it is necessary to bear in mind the essential conditions under which laryngoscopy is practised. These are—1st. The visual ray from the observer's eye has to be directed to a part which, when the mouth is open, is hidden from direct view by the projecting angle formed by the base of the tongue and the epiglottis. 2nd. The part to which the visual ray is directed—that is to say, the larynx—is not penetrated

by light. To overcome these obstacles to laryngoscopy there are required, then (1), a small mirror placed at the back of the throat at such an angle that the visual ray can be directed into the larynx; and (2) an apparatus for concentrating the luminous rays, and projecting them upon the small mirror, so that they can be reflected into the larynx. For this latter purpose modern laryngoscopists generally use a second mirror (the ophthalmoscopic mirror of Rüete). The essential requisites of a laryngoscope were clearly perceived by Dr. Babington as early as the year 1829. At that period his inventive genius was directed to the subject, and he contrived a laryngoscope very similar to the one now in use. It consisted of a small laryngeal mirror, and a hand-mirror for concentrating the light. The patient sat with his back to the sun, and while the illuminating mirror (a common back-hair glass) was held with the left hand, the laryngeal mirror was introduced with the right. Dr. Babington's labours did not end here. By a very simple mechanism, a tongue-depressor was united with the laryngeal mirror, and thereby one of the most serious obstacles to laryngoscopy was attempted to be overcome. This ingenious laryngoscope [instrument shown] was exhibited at the Hunterian Society on March 18, 1829; and a report of the meeting was published in the *London Medical Gazette* of March 28, 1829. It was made by an optician named Elsworthy, and the invoice [shown] was dated June 23, 1829. In this laryngoscope a spring between the shanks of the laryngeal mirror and the spatula was fixed in such a way that by pressing the two handles together the tongue was depressed. Afterwards (between the years 1830 and 1835) Dr. Babington gave up the tongue-depressor, and had two instruments made very similar to those now in use, except that instead of being of glass they were of steel. The maker's name (Laundy) is stamped on one of the mirrors. In rendering justice to a distinguished physician, the author did not wish to detract from the credit fairly due to others who, at different times and independently of one another, had contrived an apparatus for inspecting the larynx. Dr. Czermak's great merit consisted in his having so simplified the instrument that it could be used by the many; and his talent and enthusiastic teaching had so prominently associated his name with the laryngoscope, that his reputation would not suffer from a due recognition of Dr. Babington's claims. The author said it could not be denied that as early as 1827 Dr. Senn of Geneva tried to make use of a small mirror to examine the larynx of a child on whom he was about to perform tracheotomy. Dr. Senn was unsuccessful because he only employed one of the factors in laryngoscopy; and whilst he attributed his failure to "the small size of the mirror," it really depended on the non-illumination of the larynx. Dr. Senn's case, moreover, was not published in the *Journal de Progrès* till the year 1829, a short time after Dr. Babington's description had appeared in the *London Medical Gazette*.

ART. 91.—Case of a Mucous Cyst on the Laryngeal Aspect of Epiglottis, seen by the Laryngoscope, and successfully treated by Incision.

By Mr. DURHAM, Assistant-Surgeon to Guy's Hospital.

(*Proceedings of the Royal Medico-Chirurgical Society*, Nov. 10, 1863.)

CASE.—The patient, a very intelligent lad, aged eleven, was admitted into Guy's Hospital, under the care of Dr. Wilks, on June 10th, 1863. He had for three years suffered from gradually increasing impairment of voice, and difficulty of breathing and swallowing. On admission all his symptoms were very severe : he complained of pain, increased by pressure, about the larynx ; he did not breathe freely ; his voice was reduced to a low whisper ; solids seemed to stick in his throat, and he could only swallow liquids with difficulty. During the night of the 14th he was seized, as he had previously been on several occasions, while asleep, with a very severe attack of dyspnoea. Tracheotomy was upon the point of being performed, but was delayed by the desire of Dr. Wilks, and on the following morning Mr. Durham was requested to make a laryngoscopical examination. On doing so, the epiglottis could not be distinguished in its normal form, but instead, there appeared a large, round, tense tumour, projecting backwards and downwards, and completely covering-in and concealing the glottis. On either side and rather behind this, portions of the aryteno-epiglottidean folds could be seen, swollen and apparently oedematous. The tumour could be just reached by the finger. Feeling certain that it contained fluid, Mr. Durham, with the concurrence of Dr. Wilks, at once proceeded to make an incision into it by means of a long, curved, sharp-pointed bistoury, partially surrounded with sticking-plaster. The incision was followed by a sudden gush of thick glairy mucus, mixed with a little pus and blood, which, on subsequent examination, proved to be precisely similar to the contents of a ranula beginning to suppurate. All the patient's symptoms were at once relieved, and in the evening he was singing in his bed. In the course of a few days he was perfectly well. Examinations were made from time to time, and it was interesting to watch the gradual subsidence of the oedema, and the return of the parts to their normal condition. The patient was last examined nearly four months after the operation ; he was in every respect well. There was no appearance of the cyst (for such evidently was the nature of the tumour), but the cicatrix of the incision could be just distinguished on the lower part of the laryngeal aspect of the epiglottis.

ART. 92.—A Dislocated Cervical Vertebra reduced successfully.

By M. RICHET.

(*Journ. de Méd. et Chir. Prat.*, Août, 1864.)

CASE.—The patient was a boy of twelve, who was admitted on the 20th of July, 1863, into the wards of La Pitié Hospital. The injury had been caused by direct violence, and had induced symptoms of paralysis, caused by undue pressure on the brachial plexus at the emergence of the nerves from the intervertebral foramina. In order more closely to investigate the case, M. Richet, with the assistance of his colleague, M. Gosselin, placed the boy under the influence of chloroform, and ascertained that spasmodic contraction of the muscles had some share in the inflexion and rotation of the head, to

which various movements were readily imparted during anaesthesia. After having thus thoroughly satisfied themselves of the correctness of the diagnosis, the surgeons proceeded as follows to the reduction of the displacement.

Counter-extension was applied by means of a sheet folded lengthwise, running behind the neck, and firmly secured to the foot of the bed. Two assistants grasping the patient's head at the back and beneath the lower jaw, performed extension, and M. Richet placed his own hands over those of the assistants, not for the purpose of increasing only, but also of directing the tractive power. He at first inclined the head towards the right shoulder, in order to liberate the inferior articular process which he supposed to have passed over the superior process of the subjacent vertebra, forcibly rotated the head from right to left, in a direction opposite to the deformity caused by the accident, and concluded the operation by turning the head and neck directly backwards.

During these manipulations, M. Gosselin exercised lateral pressure on the cervical column, and endeavoured to push back the displaced vertebra into its normal situation.

The operators proceeded with the greatest caution and deliberation, and they already fancied that the straightening had made obvious progress when they became simultaneously aware of a sudden jerk ; M. Richet, somewhat alarmed, immediately caused all effort to be discontinued, but soon ascertained with much gratification that the inflexion of the spine and the deformity of the neck had almost entirely disappeared. The child's face was now directed forwards, the chin occupying the mesial line, although the head remained almost imperceptibly bent forward.

The case has since progressed most favourably, all signs of paralysis have disappeared, and, with the exception of slight deviation of the head, which may depend on some degree of arthritis, occasioned by the unavoidable efforts used during the reduction, the child is doing well.

It is true, that in the present instance it was highly desirable to relieve the paralysis of the arm and thoracic walls by reducing the dislocation, but nevertheless the very hazardous operation which terminated in so satisfactory a manner, was instituted with great caution and only after much hesitation. The exhibition of chloroform proved extremely useful ; thanks to anaesthesia, the anatomical condition of the parts was carefully inquired into, and it became possible to dispense with any violent tractive efforts, which the powerful contraction of the numerous muscular structures which surround the spine would otherwise have rendered necessary.

ART. 93.—Spontaneous Dislocation of the Two Upper Cervical Vertebrae, with complete Paralysis of the Limbs and Trunk, cured by reducing the Dislocation.

By M. MAISONNEUVE.

(*Gazette Hebdom. de Méd. et de Chir.*, Juillet 8, 1864.)

CASE.—Marie-Louise Paquette, aged 16, suffering from white swelling of the atlido-axoidian articulation, of several months' standing, was admitted into the Hôtel Dieu, March 24th, 1864. There was swelling of the sub-occipital region, the head was inclined forwards, and there was slight numbness of the upper limbs. On the very day of her admission, in consequence of a sudden movement of her head, the two upper cervical vertebrae were dislocated, and there instantly followed paralysis of all the limbs and the trunk. The diaphragm alone kept up the respiration.

Reduction was attempted by M. Maisonneuve, by placing one hand under the chin, the other under the occiput, and by then pulling up the head gently and continuously whilst the shoulders and trunk were held down by two assistants. In about half a minute a slight start and a very distinct friction-sound showed that reduction had been successfully accomplished. Sensibility and even motor power began to return almost instantly. Great care was taken to keep the head in proper position ; the improvement increased, and on the following day the paralysis had almost entirely disappeared. Eight days later there was no trace of it left.

(B) CONCERNING TRUNK AND ABDOMEN.

ART. 94.—*Gastralgia, an Initial Symptom of Caries of the Vertebræ.*

By DR. BENJAMIN LEE.

(*Canada Lancet*, August 15, 1864.)

"My attention," writes Dr. Lee, "has been so frequently called of late to an important, early, and characteristic symptom of spinal caries, that I feel at liberty to claim for it a more careful consideration on the part of the profession than, I am convinced, it has heretofore had given it.

"I refer to acute, paroxysmal, and often excruciating, pain. This pain originates in the majority of instances at the epigastrium, less often at the umbilicus, or between these two regions, and in the smallest number of cases in one side or the other. It is almost invariably the first symptom of commencing caries, or perhaps I should more correctly say, of the inflammation, whether of the intervertebral cartilage or of the periosteum, which precedes the caries. Unhappily our pathology is not yet sufficiently advanced to enable us to say with confidence what the first organic change is. Whether the disease have a traumatic origin in a perfectly healthy system, or is the result of a vice of constitution, the fact is still the same, that in nine cases out of ten it is ushered in by long-continued and oft-repeated attacks of gastralgia.

"The point at which the disease is situated exerts a modifying influence, the middle dorsal being the region in which the affection is most characteristic, and more apt to be confined to the epigastrium; but at no point is there entire immunity.

"This pain does not take its starting-point at the seat of disease and radiate towards the anterior surface of the body, but, as I have stated, *originates* in front. The length of time during which the patient suffers from it before the ulcerative process has destroyed enough of the substance of the bone to produce actual and unmistakable deformity is variable; but it has been noticed not unfrequently six months, and in some rare instances an entire year previous.

"So constant is this phenomenon, that out of nearly a hundred cases which I have examined during the past year, I do not think that half a dozen failed to present it; and in some of these there

was an entire absence of constitutional symptoms ; for, strange to say, the disease may, in some rare instances, go on to produce very marked deformity, without apparently affecting the general health. . . .

" Lesions involving nervous centres express themselves often, perhaps usually, through the general system rather than locally. Let him, therefore, carefully scan the carriage and gait of his patient. If he turn the toes in, if he hold the trunk slightly bent forward, and rigid, as though apprehensive of a concussion or jar, if he refuse to bend the back in stooping to touch the floor, then there is undoubtedly mischief going on between some of the vertebrae. But he may not yet feel satisfied without some 'ocular demonstration.' Let him strip the patient's back, and place him in good light. Let him examine first laterally. If he find at any point, in the spine, an angle, not necessarily a projection, but simply an angle, in place of the normal curve, he has found the seat of disease. This failing, let him take the full view of the back. If there be a lateral deviation of the spine, and that deviation present not a curve but an angle, he has then an evidence of *angular curvature* (so called) of the spine, the early diagnosis and treatment of which may be of the utmost importance to his patient."

ART. 95.—Cases of Strangulated Hernia, treated by Inflation of the Bowels, and by shaking the Patient whilst in an Inverted Position.

By Mr. RICHARD GRIFFIN, Weymouth.

(*British Medical Journal*, Oct. 22, 1864.)

CASE 1.—A short time since, I visited, in consultation, a female, with symptoms of strangulated femoral hernia, which had existed for a couple of days. In consequence of several attempts at reduction, the hernia, about the size of a walnut, had become very painful, and there was some tenderness of the abdomen ; vomiting was frequent, and there was no action of the bowels, although several large aperient doses and enemata had been administered. I procured a pair of bellows and passed the nozzle into the anus, an assistant holding the nates well together, so as to prevent, as much as possible, the escape of air from the bowels. The patient complained of the distention giving her pain ; but the inflation was continued for about a quarter of an hour, much of the air escaping either through the bellows, or by the side of the nozzle ; but it was, apparently of no benefit.

A fresh consultation was then held ; when it was deemed advisable to recommend an operation. This, however, was postponed for a short time, but fortunately was not needed. In about an hour after inflation, the bowels acted freely, and all the symptoms of strangulation ceased ; the distension having caused the forcible withdrawal of the strangulated knuckle of intestine which had prevented the peristaltic action of the bowels. The patient recovered.

CASE 2.—In another case, to which I was called in consultation, the old woman had stercoreaceous vomiting for three or four days, with constipation. The hernia was femoral and irreducible. The bellows were used in this case, as in the last ; and, although they appeared at the time to have been of no avail, yet, in about an hour, a free action of the bowels took place. Un-

fortunately, however, there was no stopping the diarrhoea which ensued, arising from the drastic purgatives which had been administered, and the old lady died in forty-eight hours. The inflation, however, reduced the hernia ; and so far was a success.

CASE 3.—I was called in consultation to a man who had a strangulated inguinal hernia, which could not be reduced by the taxis. Purgatives and enemata had been freely administered during the preceding three days. I at once tried the bellows ; which had not been used many minutes, before the man called out, " You are blowing up my purse !" Such was, indeed, the fact ; the scrotum having become largely distended with air, owing to a laceration in the rectum having been made with the pipe of the enema-syringe, which, I subsequently ascertained, had been so roughly used, that the man called out when the enema was being administered, and blood followed the withdrawal of the pipe. The bellows were removed, and the man immediately went to the night-stool ; and a copious evacuation followed. I did not see the patient again ; but I subsequently heard that he got well, and the air in the scrotum was soon absorbed.

CASE 4.—A fourth case had been related in one of my poor-law pamphlets, in which a strangulated hernia was reduced by the bellows ; the taxis, enemas, and purgatives having previously failed.

These cases prove that, before the operation for strangulated hernia is performed, it would be well to try the effects of inflation ; although this, like everything else, is sometimes liable to fail, as the following cases will prove ; superadded to which, shaking the patient whilst in the inverted position also failed.

CASE 5.—A man with scrotal hernia, to whom I was called in consultation, had all the usual remedies tried, including inflation of the bowels, but in vain. He was then hoisted with his knees over my medical friend's shoulders, and lifted in that position until only his head touched the bed, and had a thorough good shaking. The intestine, however, was too tightly held to be dragged from its position by this procedure ; he was, therefore, obliged to submit to an operation, by which he was cured.

CASE 6.—A lady, very stout, about fifty years of age, who had suffered for years with an irreducible umbilical hernia, for which she wore a supporting abdominal-belt, having one day used little extra exertion, came home fatigued, and complained of pain in the bowels, followed by sickness and constipation. Enemata, salines in a state of effervescence, then opium, and finally purgatives, with inflation of the bowels, were tried, together with the taxis ; but the hernia could not be reduced. A former medical attendant of the lady was then summoned from a distance ; and, on his arrival, he suggested that shaking in the inverted position should be tried. Accordingly, the patient's knees were placed over his shoulders, with her legs hanging down his back, and, with the assistance of several persons present, he gradually raised himself into an upright position, the patient's head only touching the bed. He then gave her two or three good shakings, which did not occupy more than a minute or two ; but, on looking at the patient's face, I discovered that breathing had ceased, and she was pulseless. We instantly placed her in the recumbent position ; the window was thrown open ; and a napkin, with the end dipped in water, was very freely applied to the face and chest, the slaps from it being anything but gentle. In a few minutes, there was a slight sigh ; and, after a short time, we had the pleasure of having our patient in no worse a state than before the attempt at reduction by shaking : but it was felt to be a painfully narrow escape by all present, and may serve as a warning, not to be disregarded by medical men, that they ought to be careful how they turn stout people topsy-turvy. A medical gentleman from Bath was telegraphed for ; but, in spite of the efforts of four surgeons

our patient gradually became worse, and died. As we were not permitted to have a post-mortem examination, the precise cause of death was not ascertained.

ART. 96.—*Radical Cure of Inguinal Rupture.*

By MR. JOHN WOOD.

(*Medical Times and Gazette*, May 7, 1864.)

Operation by Pins.—Mr. Wood uses this operation in cases of congenital hernia, and those of small size in lads and young men. In such cases, when the hernial canal is narrow, its sides yielding, and easily and completely approximated, there is no need of any intervening substance to fill up the hernial gap. The tissues also are thin and comparatively unaltered, and permit of an accurate examination, by the finger, through them when invaginated into the canal.

The apparatus used in this operation is very simple, and no incision through the skin is required.

The pins employed are of various sizes—from three to five inches long—hardened at the point, but made in the shaft so that they will bend a little rather than break off short.

FIG. 1.



They are spear-pointed, curved boldly near the point like a suture needle, flattened on the concavity, with slightly cutting edges, and with a rounded shaft. At three or four inches from the point the shaft is bent into a right angle and twisted into a loop large enough to receive the point of a fellow needle. The projecting end is about an inch long, terminating in another loop for the purpose of being held by the dressings. See Fig. 1. When applied, the pin should be held by the middle of the shaft between the thumb and long finger, the end of the forefinger being placed upon the bent extremity. They are applied in opposite directions through the structures intended to become adherent, and are pinned through and through like an ordinary toilet-pin. Each point is then passed through the angle loop of the fellow pin, and the ends then rotated in opposite directions, so as to twist up the confined tissues, and fixed by straps of plaster and pads.

The patient being laid on his back with the knees and shoulders raised, the scrotum is first invaginated into the canal, and the relative positions of the cord, the conjoined tendon, and Poupart's ligament carefully noted. In small cases this can be most conve-

niently done with the little finger. The finger should be carried as far up as possible, so as to fill in front of it the lower border of the internal oblique muscle, and to the inner side of it the conjoined tendon. The skin of the groin being then drawn directly inwards by an assistant, the needle is carried with its concavity outwards and downwards through the tissues directly upon the inside of the nail of the invaginating finger (see Fig. 2). The point then turned

FIG. 2.



downwards towards the pubis, and pushed steadily after the invaginating finger as it is slowly withdrawn, protecting the needle on the outer side. As the needle passes over the pubis it is made to take up the fibres of the internal pillar attached to that bone, grazing the bone as it does so. The needle and finger must be kept fairly together, the last two fingers of the hand resting firmly upon the groin over the deep ring. This gives steadiness to the hand, and, at the same time, by its pressure, keeps the bowel out of the canal. The point of the pin is then to be cut off with a pair of pliers. The second pin is then taken, and its point, with the concavity directed forwards, passed into the aperture through which the first emerges in the scrotum. The invaginating finger is then placed below it, and passed up into the canal, carrying the pin before it till it touches the middle of the back surface of Poupart's ligament. Through this the pin is then pushed upwards and outwards till it can be seen to raise the skin (see Fig. 3). This is then drawn inwards by the assistant until the point of the pin can be pushed through at the same place at which the first pin entered. The pins are then locked, after which the point of the second pin is to be cut off conveniently close, and one of the bent ends of the pins twisted over the opposite side and held down by a pad of lint and a strap of adhesive plaster. A strip of oiled lint is then twisted round the pins where they lock, so as to protect the skin from their pressure somewhat (see Fig. 4). The whole is then retained by a pad and spica bandage.

In this operation the conjoined tendon and inner pillar are transfixed by the first pin, which thus traversed the inner and hinder part of the hernial canal and sac, while the outer pillar is fixed at

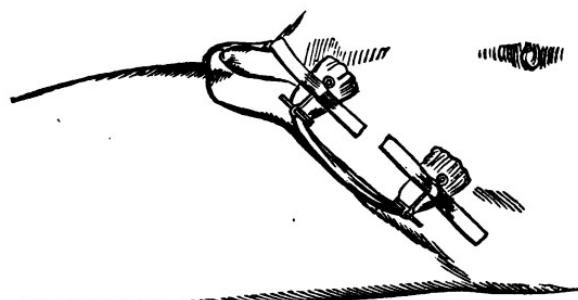
Poupart's ligament by the lower pin. The whole of these parts, with the hernial sac, are twisted together by the revolution of

FIG. 3.



one pin round the axis of the other, so that the hinder wall is drawn forcibly forwards and outwards, and the front walls back-

FIG. 4.



wards and inwards till the canal is firmly closed. The cord lies behind the pins, closely embraced by the twisted tissues. Adhesion takes place between the parts in their new position, following up closely (if the case does well) the slow ulceration in the course of the pins. These are kept in a week or ten days, according to the amount of fibrinous effusion which takes place in the canal. About that time a thick firm mass of deposit occupies the course of the pins, excluding the bowels, filling up the sac, and uniting all the twisted tissues into a solid mass. When the pins are withdrawn, simple water dressing or a poultice may be applied. As the wound heals, a stout pad should be applied over the dressings, and held in its place by a bandage firmly applied. When the openings have sufficiently healed, a horseshoe-pad truss should be applied, with a bit of cotton wool or lint between it and the skin to protect the tender skin. Until this time the patient should be kept in the recumbent position. After the truss has been applied, he may be suffered to get up.

If, in the course of the treatment, the testicle becomes much swollen, all bandages and pressure should be removed, and cold water or evaporating lotion applied. Some degree of swelling of the testicle is beneficial, rather than otherwise, as showing a sufficiently close embrace of the cord, and, consequently, obliteration of the canal. The pins usually are withdrawn with great ease and very little pain. The patients ordinarily suffer so little, that food is taken in the usual way a day or two after the operation. It is seldom necessary to employ a stricter regimen than milk diet for the first few days. As the wound heals, an allowance of wine or beer may be permitted, and the general health of the patient supported as much as possible, to ensure a more vigorous reparative formation in the parts.

In some cases the pins are applied transversely to the canal, especially where, as in cases of a large size and direct kind, the rupture shows a tendency to emerge behind the cord, lifting it forwards and pushing it outwards. In some of these very large cases it has been necessary to repeat the operation in order to meet this tendency. Wire has also been applied transversely under these circumstances. It is important in such cases to make the deeper pins or wire touch or graze the upper border of the pubis, over which the rupture emerges. By establishing a bridge of adhesion there, this tendency is better counteracted.

In the case of a child, aged three years, a very large rupture, reaching half-way down the thigh, and totally unmanageable by any kind of truss, has been very successfully treated. No impulse whatever is now felt on crying, and the large sac can be felt doubled up in the scrotum, and forming a hard tumour, connected above with the pillars of the ring and canal. The child's health and appearance have positively improved during the time that he has been under treatment. Mr. Wood has found it very advantageous in this and other large cases to keep the child's head and shoulders laid a little lower than his body and legs. This diminishes much the force with which the rupture presses from above upon the newly-formed adhesions. In these large cases the ring truss pad will be found to give a more direct and efficient support to the adhesions. At the same time it does not press upon the structures which fill up the hernial gap in the abdominal walls. The sides of the inguinal canal and the pillars of the superficial ring, already closed up by the operation, are kept in position by the pressure of the ring or horseshoe-truss pad. The first complete closure and obliteration is effected by the operation, and the truss, by supporting properly the newly-formed tissues, during some months after the operation, completes the cure.

Mr. Wood has kept cases treated upon this plan in view for two or three years, the rupture remaining perfectly cured up to the present time, without any truss being worn after the first six or eight months. The growth and development of young subjects assist powerfully in rendering the resistance to reprotrusion more efficient as time elapses, during which the bowel is effectively excluded from the canal.

**ART. 97.—Four Cases of Intestinal Obstruction, in three of
which the Colon was opened in the Left Lumbar Region
by Amussat's Operation.**

By Mr. SAMUEL SOLLY, Surgeon to St. Thomas's Hospital, &c.

(*Proceedings of the Royal Medico-Chirurgical Society*, April 12, 1864.)

After acknowledging the value of Mr. Cæsar Hawkins' contributions, Mr. Solly detailed his own experience. The operation of opening the colon in the loin, in properly selected cases, he believed to be neither dangerous nor difficult; nor was it contraindicated by the occasional passage of small quantities of faecal matter. The results, moreover, of the operation were far from unfavourable, even where the stricture was due to malignant disease; and the relief afforded was instant and decided.

The first case, that of a labourer, aged twenty-eight, was one of rectal cancer, advancing with unusual rapidity, and dating only five months from admission. At the onset, diarrhoea, with slimy and bloody motions, was followed by intermittent symptoms of obstruction, which before long became constant. On admission, the abdomen was frightfully distended; the rectum did not admit the passage of even a small elastic catheter. Three days afterwards, the descending colon was opened in the left lumbar region. Much flatus escaped, but hardly any faeces until some hours after the operation. This was followed by rapid improvement in every respect. Sixteen days later the patient left his bed, wearing a tent in the wound. He continued in fair health for three months, when chronic peritonitis set in, probably from extension of the primary disease, and slowly advanced to a fatal termination.

The second case, that of a railway clerk, was of eighteen months' standing. It had been very gradual in its access, and palliated during the last month by the passage of rectum-bougies twice a-week. On admission, the symptoms, both local and general, were very severe. At the operation, much faecal matter was evacuated, and the relief obtained was very decided, lasting for six weeks, when sudden collapse, partially due to mental causes, was very rapidly followed by death. Post-mortem examination was refused.

The third case had occurred very recently. A bank cashier, aged fifty-four, of active and temperate habits, had begun about a year previously to suffer from constant tenesmus, with from five to seven evacuations daily; these were on three or four occasions accompanied by blood. Seven months later he consulted Mr. Solly, and extensive carcinoma was found, partially occluding the bowel. During the following five months he could evacuate the bowel without much distress; but at the commencement of the present year complete obstruction took place, accompanied by great distension of the abdomen, for which, ten days later, he was admitted into St. Thomas's Hospital. On admission he complained of dyspnoea and retching, but without actual vomiting. A nodular mass could be felt projecting into the rectum, hard and inelastic, almost ob-

literating the canal. The following day, a horizontal incision with a slight inclination upwards and outwards, from three inches and a-half to four inches long, was made midway between the last rib and the iliac crest. The erector spinae was partially divided, and then the quadratus lumborum inwards, on a director, to the extent of an inch and a-half. The abdominal fascia was similarly treated, leaving the bowel exposed; this was fixed by means of two silk sutures to the edges of the wound, and the gut opened transversely by means of scissors. Very little blood was lost, and three pints of liquid faeces came away with intense relief. During the afternoon more liquid faecal matter was discharged *per anum*. On the following day the pulse was 86; he had slept well, and eaten a chop for dinner. The wound showed no inclination to close, and slight prolapse of the bowel was easily checked by an oiled sponge-plug. For six days he did very well, except that there was a tendency to the formation of bed-sores. Thirteen days after the operation he sat up two hours daily, and seemed better, faeces passing both by the wound and anus. But on the twenty-first day collapse, vomiting, and swelling of the face came on, followed by great dyspnoea and extensive mucous crepitation in the chest, which increased so rapidly as to carry him off in a few hours. At a post-mortem examination, tubercle was found in both lungs, which were œdematosus, with much injection of the bronchial tubes. The cæcum was distended by gas, and was adherent to the sheath of the right psoas muscle, in which was an abscess extending to its origin, containing several ounces of thin fetid pus. There was a free opening in the descending colon, where it was uncovered by peritoneum, and its edges were adherent to the wound. The sigmoid flexure contained solid faecal matter. The rectum was embedded in a firm mass of infiltrated tissues, adherent to the pelvis posteriorly and to the bladder in front. The deposit was firm, fibrous, and juicy. A softer growth projected inwards round the whole circumference of the bowel, so as almost entirely to obstruct it; below the tumour was an ulcerated surface of the intestines, two or more inches across, where the coats of the bowel were entirely destroyed. The scirrhus mass under the microscope showed fibrous tissue, with cells of various sizes, mostly round, and containing minute oil-globules. The liver contained several masses of scirrhus, most of them presenting traces of central degeneration.

The fourth case resembled the preceding up to a certain point, where it diverged in consequence of the operation not having been performed, and the patient dying rapidly from rupture of the bowel. W. C.—, aged fifty-five, a surgeon, was a ruddy and robust man, of very active habits, subject to no illness except occasional attacks of gout. In April, 1863, he had a severe attack of what he termed colic, for which his father-in-law, a retired practitioner, successfully treated him. Even at that period Mr. Solly saw reason to diagnose stricture of the large intestine, though the patient attributed his symptoms to gout, and did not formally request his advice until two months later. On a first visit he was in great pain, and the abdomen was so tender as to preclude close examination. Palliative

treatment mitigated the symptoms for a fortnight, and then the author was again hastily summoned to Tunbridge Wells. Examination of the rectum by means of the finger and a soft bougie revealed an obstruction at its upper part. The abdomen was intensely distended with flatus, although small quantities of faecal matter were once or twice evacuated. It was suggested to open the descending colon in the left lumbar region, but postponed for the opinion of an eminent surgeon, who came to the conclusion that it was not desirable. Accordingly Mr. Solly unwillingly relinquished the proposal under protest. Within twenty-four hours the patient died in great agony, with symptoms of ruptured bowel. On post-mortem examination, the abdomen was tense, and prominent above the umbilicus. The lower part of the descending colon presented a small perforation, from which half-an-ounce of faecal matter had escaped. There were no signs of recent peritonitis; and the intestines were healthy except at the sigmoid flexure, where a mass occupied the interior of the gut, of a scirrrous hardness, measuring over three inches across. Several pulpy lobulated portions of this occupied the calibre of the tube, fitting into each other like cogs of wheels. No other morbid condition was found. Before opening the abdomen a trocar had been introduced at the spot where the proposed operation would have been performed, and its point was found in the intestine an inch and a-half above the diseased mass.

In commenting on this case, it was noticed that death had obviously been the result of obstruction, and not of any rapid malignant growth, and that life would undoubtedly have been prolonged by the operation for an indefinite period. Moreover, the valvular form of the obstacle was pointed out, and the possibility of the original channel becoming pervious when the accumulation above was removed by operation, as had actually occurred in the third case. Lastly, attention was directed to the fact, already noticed by Mr. Quain in his work on this subject, that obstruction might be complete and prove fatal, even when the autopsy did not exhibit total oclusion of the intestinal cord.

ART. 98.—*On some Points in the Treatment of Diseases of the Rectum.*

By Dr. VAN BUREN.

(Amer. Med. Times, May 7, 1864; and Med.-Chir. Rev., Oct. 1864.)

Dr. Van Buren, after adverting to the necessity of bringing the patient under complete anaesthetic influence before introducing the speculum, describes the position which he regards as best suited for this purpose, and which is, in fact, that employed by Dr. Marion Sims in his operations upon the vagina. “The patient is placed with the upper part of the trunk in a prone position, the front of the thorax in contact with the bed or table, the head on its left side, the left arm brought out at the left side and carried behind the back, the pelvis on its left side, with the legs flexed at a right angle, and

the buttocks towards the light, and elevated sufficiently to permit the abdominal viscera to gravitate towards the diaphragm The speculum I employ is a modification of that used by Dr. Sims for the vagina, the modification consisting in the notch at its angle intended to receive the sphincter muscle, and thus to resist the tendency of the instrument to slip out when the muscle is put upon the stretch, and the alteration of the handle, which is so shaped as to clear the buttock when it is swept round in order to bring all sides of the cavity of the gut into view. To facilitate still farther this manœuvre, and to protect the stretched mucous membrane from abrasion, the edges of the blade are carefully rounded and turned inwards. The speculum being introduced, and the sphincter put upon the stretch with one hand, the other hand is used to draw the parts away on the opposite side of the orifice; and the handle of the instrument being then gradually swept around, the light falls successively upon the exposed and expanded cavity. By the use of this manipulation, applied in the position just described, I have frequently obtained a satisfactory view of the whole extent of the cavity of the rectum as high as its upper curve."

For the relief of *fissure*, or rather *irritable ulcer of the rectum*, as Dr. Van Buren prefers terming it, he regards forcible dilatation of the sphincter as an operation justified by theory and approved by practical results, and as one which should always be substituted for the knife. "Forcible dilatation, as I have been in the habit of performing it—by introducing the two thumbs into the anus, flexing them so as to include the breadth of the sphincter, and then, taking a purchase with the outstretched hands from either buttock, drawing them forcibly asunder until arrested by the ischial tuberosities—effects such a stretching of the fibres as to paralyse the sphincter for at least a week, during which time the ulcer assumes a healthy appearance, and rapidly heals, the pain ceasing entirely from the time of the operation I have been asked whether the paralysis of the sphincter produced by dilatation is ever followed by incontinence or loss of control over the contents of the lower bowel. The relaxed and flabby appearance of the orifice after the operation certainly suggests the idea; and the fact that when the patient is asked to contract his sphincter by voluntary effort he generally expresses his inability to do so, looks in the same direction. But in upwards of twenty cases which have come under my observation, I have never seen any indication of incontinence, and I believe the internal sphincter to be equal to any emergency likely to arise during the temporary suspension of the functions of the more powerful external muscle. I have reason to believe that in some cases the muscular fibres of the sphincter are actually ruptured in the stretching process; but no harm or delay in the cure has arisen from this.

"This remedial measure has a wider application than to the treatment of irritable ulcer. In inflamed haemorrhoidal tumours, or any painful inflammatory affection of the anus, where the spasmotic contractions of the sphincter constitute the principal source of the pain and obstruction of the circulation, the stretching of the sphincter, with

or without the intervention of an anæsthetic, will afford prompt and certain relief. It is equally applicable to the affection described by some authors as 'spasm,' and by others as 'painful contraction' of the sphincter, and also in the 'neuralgia of the anus' of nervous subjects and hysterical women. I have been in the habit for some years past of employing this manœuvre after the operation for the cure of hæmorrhoidal tumours, whether by ligature or the use of the écraseur. The result has always been favourable. The patient is entirely saved from the severe pain, generally lasting several days, which is caused by the pinching of the tender and inflamed parts by the spasmoid contraction of the irritable sphincter. By throwing it out of play, the suffering after the operation is reduced to a very moderate degree of local soreness, and the necessity for the employment of the catheter through sympathetic disturbance of the sphincter of the bladder is entirely abolished."

ART. 99.—*Case of Unusual Difficulty in Lithotomy, arising from Great Distortion of the Pelvis by Rickets.*

By Mr. HENRY THOMPSON, Surgeon to University College Hospital.

(*Proceedings of the Royal Medico-Chirurgical Society, Nov. 24, 1863.*)

In this case there was very considerable obstruction to the removal of a stone from the bladder arising from the pelvis being extremely narrowed and deformed by rickets. The author is not aware of any similar case in the records of lithotomy.

CASE.—G. S—, aged four and a half years, was first seen by Mr. Thompson, August 28th, 1863. The symptoms of stone were severe. The child was much emaciated. A cicatrix existed in the perineum, with a fistulous opening, showing the site of a median operation for stone performed by Mr. Erichsen about ten weeks before, on which occasion several portions of the outer layers were renoved. The stone was grasped, but the forceps could not be removed with it; and owing to the condition of the child it was deemed at that time prudent to desist, and postpone the attempt. The wound healed kindly up to a fistulous aperture, but diarrhoea came on, and the child left the hospital to recruit its strength.

At the time of its readmission, Mr. Erichsen, being from home, authorized Mr. Thompson to do what he considered necessary with the case. The sound passed easily into the bladder, and a large stone was detected. The child was admitted into University College Hospital, August 31st, and was placed on the operating-table September 2nd. Mr. Thompson made his incisions in the old track, and, introducing his finger, recognised the sacral promontory as remarkably prominent; beyond this the finger passed with difficulty, flatwise, and touched the end of a stone lying in the bladder, which was altogether in the abdomen. There was no room to pass lithotomy forceps up by the finger. Finding, then, by placing the right hand on the abdomen, that the stone was easily felt there, and could be pushed down to the upper outlet of the pelvis, Mr. Thompson directed his assistant to maintain it firmly there by pressure, while, with a slender-bladed polypus forceps, he extracted the stone with some difficulty entire. There was no

bleeding of any consequence. Peritonitis set in next day, and proved fatal in three days.

At the post-mortem the pelvis was removed : this was exhibited to the Society. The deformity was very remarkable. In the dried bone, when the soft parts were taken away, only sufficient room existed for the stone to pass through in its long axis, the dimensions of the outlet only just exceeding the smallest diameter of the stone. The upper opening of the pelvis is obliquely caudate ; the sacral promontory being within $\frac{1}{8}$ inch from the left pubic ramus, and barely $\frac{1}{8}$ inch from the right : from the pubic symphysis it is nearly $\frac{1}{2}$ inch. The stone is hard uric acid, of a flattish oval form. It is $1\frac{1}{2}$ inch in length, $\frac{1}{2}$ inch in breadth, and $\frac{1}{8}$ inch in thickness.

ART. 100.—Two Cases of Stone in the Bladder of the Female treated by Rapid Urethral Dilatation, with Remarks on the Operation.

By MR. THOMAS BRYANT.

(*Proceedings of the Royal Medico-Chirurgical Society, Aug. 26, 1864.*)

In a paper upon this subject Mr. Bryant gives the history of two cases, of which the following are brief notes :—

CASE 1.—H. C—, aged fifty-two, admitted October 13th, 1862. She had had symptoms of urinary irritation for eight months, and for three had been quite unable to retain her urine. On November 19th the stone was removed by rapid urethral dilatation, and it measured $1\frac{1}{2}$ inch by 1 inch in diameter. The operation was followed by immediate relief. On the day following the patient could hold her urine for twenty minutes, and in a week for many hours, and in three weeks she left well.

CASE 2.—Ann C—, aged thirty-five, was admitted on February 17th, 1864. Urinary symptoms had existed for seven months, and were very severe ; the urine was full of pus, mucus, and was intensely fetid. The least examination caused her great pain. After rest in bed for several weeks these symptoms had improved, and on March 5th the operation was performed. Chloroform was given, and the urethra rapidly dilated by Weiss's dilator. The stone was then removed, but little difficulty being experienced in its removal. The calculus measured 2 inches by $1\frac{1}{2}$ inch in diameter, and $5\frac{1}{4}$ inches by $4\frac{1}{2}$ inches in circumference, weighing 2 oz. 2 dr. The next day the patient could hold her urine for fifteen minutes, on the second for forty, on the third for two hours and a-half, and on the fifth day for five hours. A small slough separated on the sixth day from the urethra, and during an examination the finger was introduced again into the bladder, which caused some little incontinence ; but this gradually disappeared, and on April 5th she was able to retain her urine for five hours. She is now convalescent, having good power over her bladder for three or four hours.

The author then passes on to the consideration of the dilatability of the female urethra, and quotes many interesting cases, which are tabulated, in which a calculus had been expelled

by natural efforts, and in which *no* incontinence of urine was the result. He then proceeds to consider the operation of urethral dilatation as a means of removing a calculus or foreign body; and gives an analysis of twenty-eight cases, which he has tabulated, in which he proves that incontinence of urine resulted in only four out of the twenty-eight examples, and that in these four the dilatation had been *slowly* performed. He then considers in detail many of the cases, and enlarges upon the best mode of performing the operation. The subject of urethral lithotomy next obtains a notice; and it is proved that an incontinence of urine is a common result of the operation—nine subjects out of twenty being left in such a condition. Removal of a calculus by lithotripsy is then touched on, and the subject of vaginal lithotomy introduced, the author indicating the exceptional cases in which either operation may be required. The following conclusions are then drawn up:—1. That the female urethra may be dilated to a considerable extent with facility, and without danger. 2. That *slow* and tedious dilatation of the urethra, by sponge-tents or other means, appears to be injurious. 3. That *rapid* urethral dilatation, with the patient under the influence of chloroform, is the safest and most expeditious method of removing all average-sized calculi and foreign bodies from the female bladder; for calculi one inch in diameter in children, and even two inches in adults, have been safely extracted by this practice. 4. That the operation of incising the neck of the bladder and urethra is one of danger, and should be laid aside. 5. That lithotripsy is a valuable operation in cases in which a stone cannot be safely removed by rapid urethral dilatation—that is, when the stone is very large, and when the bladder is healthy; and that it is the best practice to remove the broken fragments by the forceps as speedily as possible. 6. That vaginal lithotomy is an operation of value when the other modes of operation are inapplicable, but that, as a general practice, it is not required.

ART. 101.—*On the Expediency of Extracting the Fragments by the Lithotrite or Scoop after the Operation of Lithotripsy.*

By Mr. FERGUSON, Surgeon to King's College Hospital.

(*Lancet*, July 2, 1864.)

The following extracts are taken from one of the Lectures on Surgery recently delivered before the Royal College of Surgeons of England:—

"I have generally, as a first step, introduced a lithotrite of considerable size, equal to a No. 10 or 11 bougie, and broken the stone into various fragments. Next, I have taken the smaller lithotrite, above referred to, attacked these fragments, and then have used the small scoop with the object of removing several fragments, so that the patient might have satisfactory evidence that the stone had been crushed. In a few days after the small crusher and scoop have

again been used—particularly the scoop, wherewith the fragments which have been found sufficiently small have been extracted singly, or two or three at a time. Thus, instead of waiting for the spontaneous escape of the fragments, a process usually both uncertain and tardy, the stone has been got rid of by direct and precise surgical interference.

"If this practice be judiciously carried out, it will, under ordinary circumstances, prove an immense advantage; and in many instances a stone may be removed with a rapidity little short of the time needful for lithotomy, with the advantage that the patient need not be confined to his bed for a single day.

"My chief object in this portion of my lecture is to draw attention to this subject. It is comparatively little known; and, moreover, a very general impression prevails that it is incorrect to extract fragments. The various instruments and mechanical devices which have been from time to time recommended or used for this purpose, or to facilitate their escape, have generally proved of so little service that they have been in a manner overlooked or laid aside by the practical lithotritist. It has even been taught that no attempt should be made to extract fragments; and, as I have already stated, so strong is the feeling in this respect, when stone in the bladder is conjoined with paralysis of that organ, that the operation of lithotrity is considered highly objectionable, if not impracticable, because there is no likelihood of the fragments passing away, excepting through a large-eyed catheter or scoop made specially for the case.

"With the instruments which it is my object to recommend, the process of lithotrity, and I believe the distress of the patient both bodily and mental, may be considerably abbreviated; and instead of paralysis of the bladder being objectionable, it is perhaps the condition most favourable to the operation, as it generally happens that with paralysis there is a callousness of the mucous membrane of the bladder which permits a freer use of the blades than under ordinary conditions.

"In early days a large instrument was thought essential for the due performance of lithotrity. The risk of bending or breaking was deemed considerable and serious, and on these grounds the largest instrument which the urethra would admit was selected for use. If a catheter or so called scoop was used afterwards, its magnitude was thought of equal importance; and to give every advantage in this respect, it was recommended that the urethra should be dilated, and, if needful, the orifice in the glans enlarged by incision, prior to the use of lithotritic instruments.

"As to the advantage of a larger urethra there can be no doubt; but I believe that large instruments are by no means so essential. Indeed I feel assured that comparatively small-sized ones are an advantage, and in certain stages of the treatment I believe them of great value.

"The modern lithotrite, particularly of the best English makers, is a very superior instrument to those in former use; and less force is required to break an ordinary stone than was imagined. We

seldom hear of an instrument of the kind either bending or breaking; and in the course of time I have come to the conclusion that one of small diameter permits of more ready manipulation than where the size is such as to fill, and be in a manner grasped by, the urethra. Hence, then, I have for many years employed lithotrites of smaller diameter than those in common use. An objection to small sizes has been made, because when the bladder is irritated to spasm the water escapes, whereas it would not if the urethra were filled (plugged) by a large instrument; but the escape can readily be prevented, if desirable, by grasping with the fingers and thumb. Even in regard to the quantity of water required in the bladder during the operation, I believe that there is no such necessity for a large amount as some have imagined. When the organ is distended with eight, ten, or twelve ounces, the stone is usually more difficult to be caught than when the quantity is smaller. The slightest touch causes it to move where there is a large amount of fluid, and fragments are certainly much more difficult to be detected and seized. No doubt it is unwise, even dangerous, to open the blades of a lithotrite freely in an empty bladder, or one with very little water in it; but I have long been convinced that there is no need for the presence of so much fluid as some have thought.

"The chief objections that I myself have found to very small-sized lithotrites or scoops are, the comparative difficulty of passing them into the bladder, and, especially, the comparative difficulty of sounding for fragments. In the finer manipulations of the sort it is hard to say whether the sense of touch or of hearing is of the greatest value. I am almost inclined to give the ear the preference; but even with this it is of the greatest consequence that the utmost facility of movement should be permitted, and hence I am averse to the use of the sounding-board, as recommended by Mr. Brooke and others, as also to the use of instruments which fill up the urethra to such an extent as to prevent the free movement of the point of the instrument within the bladder.

"I was so early impressed with the necessity of having a narrow shank for a sound, so that every facility should be given to movement, that I had Heurteloup's sound, as it is called, modified accordingly. The shank was made of smaller diameter by several sizes than the curved end, and thus the latter was a sort of lob which could be turned about readily in all directions, while its comparatively large size gave more certainty to both hand and ear. An instrument of the kind has been depicted in the later editions of my work on Surgery, and is now in considerable general use. A sound of this sort is of far greater value than one with a thick stem, or with a slender stem and equally slender point; and, in accordance with my views on these matters, Mr. Matthews has lately constructed, on his own device, a lithotrite with a narrow stem and lob-point, which, in my opinion, goes far to facilitate the effective use of the lithotrite in ordinary cases of stone. The force is applied by the rack and pinion, in accordance with my own views on this subject, but of course the same shape will answer for the various ways in which the screw-force has been so admirably applied in

modern times by Messrs. Weiss and Mr. Coxeter. The narrow shank permits free movement both in the urethra and bladder, and increased bulk at the end intended to touch the stone, gives development to both touch and sound.

"Generally when a stone is crushed the fragments are left to chance. So-called scoops and big-sized catheters have been introduced, and by the natural stream, or the force induced by injections, the fragments have been extracted, and have occasionally appeared in quicker time than if left solely to chance; but in the majority of instances the surgeon and patient have waited for the chance escape of the fragments without instrumental aid.

"At this date, there is ample experience to show that after a stone is crushed by one or repeated sittings, the fragments do come away, and a perfect cure is the result. But in many instances the last fragment is tardily discharged, and many weeks or many months elapse ere the cure is complete.

"It may be justly said of successful lithotomy that neither weeks nor months are long in comparison with the advantages gained; but, for the credit of surgery, it may be claimed as an advantage to do that in a few minutes which nature might take weeks or months to accomplish.

"The chief object I have in view in these remarks is to establish this practice.

"Supposing a stone crushed and its fragments comminuted, my proposal is, that these should be removed at the same time, or as soon thereafter as may be. The lithotrites generally in use are so large that they can be withdrawn from the bladder only when shut. If a fragment rests between the blades, it must be comminuted before these can be closed. The same remarks are applicable to ordinary scoops. Comminution must be so effectual that the blades may be closed, or nearly so, and all that can be brought away is the small bruised portion held in the hollow between the blades.

"With a small lithotrite and scoop such as I am in the habit of using, a fragment of considerable size may remain betwixt the blades, and yet the united size or diameter may readily pass or be drawn along the urethra.

"In the last sixty cases I have adopted this practice generally, and, with few exceptions, have had every reason to be satisfied. Occasionally, when over-anxious for a rapid cure, I have extracted fragments rather too large to come readily along the urethra, particularly in the prostatic or membranous portion, or at the triangular ligament. In some, when the urethra nearest the neck of the bladder has been rather roughly used, there has been considerable irritation; in others, even under such circumstances, there has been no irritation whatever; and in many instances I have been able to effect in one or two operations within ten days what, according to custom, would take weeks, or possibly months. I have done, in fact, by a precise surgical manipulation, that which according to ordinary rule is left entirely to chance. Experience has taught me that it is almost hopeless to trust to chance in all such cases; that

in many instances the fragments may be weeks, or months, in coming away, even with attempts to coax them through catheters with large eyes and other instruments devised for the purpose. Here are the fragments of stone crushed in a man whose bladder acted regularly, yet only a few of these passed spontaneously; all the rest were removed by the scoop in question in three or four operations. From first to last there was not a single bad or even troublesome symptom. The patient was detained scarcely an hour in bed beyond his regular period of rest. I never saw one suffer less distress; yet before he came under my notice, he had been strongly urged by an eminent surgeon to submit to lithotomy.

"It is considered an extreme misfortune for a patient to have stone in the bladder and paralysis of that organ at the same time; and it has been well-nigh a rule in surgery that lithotripsy is scarcely eligible in such cases, on the ground that the fragments are not likely to be expelled or carried off by the stream of urine through the urethra or a catheter. Of course I am aware that there have been successful cases of the kind; but the success has been more from lucky chance than from skilful surgical interference. And my object in these remarks is to claim for surgery a direct, special, and precise mechanical movement whereby that which has heretofore been left to chance is converted into a certainty. Here are the fragments of a large stone from the bladder of a patient who could not pass a drop of water without the aid of a catheter. All of them, with a few minor exceptions, were extracted with a small scoop. Here are the fragments—from a similar case, where, however, the urethra was unusually large—which were all extracted by a small scoop by my friend Mr. Henry Smith. The largest of these fragments is about three-quarters of an inch long by about half an inch wide, and it was extracted without any laceration or marked distension of the urethra; yet through the neck of the bladder, or a urethra of this size, even the urine would not flow.

"The mechanical development, if I may so call it, of stone shows the uncertainty of its spontaneous expulsion from the bladder. Most calculi have each a nucleus far smaller than the diameter of the urethra, but how few such nuclei pass? At all events, stones larger than the diameter of the urethra—nearly all the instances in which lithotomy and lithotripsy have been performed—are examples where small round bodies (the nuclei) have not passed away spontaneously.

"Now, I claim for surgery the power of taking away such bodies. I do not profess originality in this respect, for we all know what was done by Sir Astley Cooper in this way, but the instrument he used in removing small calculi—gravel, we might say—is of inferior mechanical powers to the modern lithotrite. It would be of little use in dealing with fragments in lithotripsy; nor would the ordinary lithotrite or scoop be of much use in what I now speak about.

"The small instruments which I now show are essential to the practice which I advocate, and with these I maintain (as I have amply tested in numerous cases) that lithotripsy can be abbreviated and brought to a certainty such as has not been claimed for it hitherto."

ART. 102.—*A New Procedure by Means of which a Passage for the Urine may be made beneath the Sub-pubic Arch.*

By M. VOILLEMIER.

(*Journal de Méd. et Chir. Prat.*, Mai, 1864.)

In the collapsed state of the penis, this organ lies in front of the pubic arch, but when drawn downwards and backwards its connections are materially altered. When the integument and subjacent adipose layer have been removed, the ligamentum suspensorium is exposed; by careful dissection, this fibrous membrane will be found to consist of two layers: the superficial and elastic portion continues above the abdominal fascia, and below is lost in the envelopes of the penis; the deeper and more unyielding layer is triangular and attached by its apex to the symphysis, and by its base to the fibrous sheath of the corpus cavernosum at the junction of the crura. On either side of the ligamentum suspensorium, immediately beneath the arch, two membranes exist perforated for the passage of nerves and blood-vessels, and behind lies a fibrous structure intended for the support of the prostatic plexus. If these various parts are removed without detaching the ligamentum suspensorium, an interval will be found between the penis and the pubic arch, gradually increasing in width as it approaches the separation of the crura of the corpus cavernosum.

M. Voillemier takes advantage of these anatomical arrangements, and proceeds as follows in his operation:—

The patient is placed on his back, the legs parted, and the pelvis raised by a cushion so as to bring forward the pubes, and prevent the prominence of the abdomen from inconveniencing the operator. An assistant, standing on the left side of the bed, draws down the penis, while the surgeon, standing on his right, ascertains exactly the situation of the ligamentum suspensorium, and inserts close to it a curved trochar, in such a manner as to pass round and beneath the pubes. In this stage of the procedure considerable attention is necessary, on account of the inclined plane presented by the anterior surface of the pubes, and of the deep situation of its inferior margin. The canula once introduced into the bladder is stopped and secured in its position.

The operation was successfully performed by M. Voillemier at the Hospital Saint Louis, on the 14th of last October. The wound healed in forty-eight hours, and nothing now remains but a fibrous band indicative of the passage of the instrument.

ART. 103.—*Absence of the Urethra in a New-born Child remedied by Operation.*

By M. PETITEAU.

(*Gaz. Hebdom. de Méd. et Chir.*, Jan. 30, 1864.)

CASE.—This case is that of a new-born child in which the urethra is described as being entirely absent. The operation, which is described below, was performed some time in August last.

The infant was placed in the nurse's lap, who with one hand secured its arms, and with the other parted and extended the legs; the surgeon then, with a narrow-bladed knife, performed an incision in the urethral depression indicating the natural situation of the meatus. A slender, grooved style was gently pushed in along the penis, and when the prostate was reached a conductor, inserted into the artificial passage along the style, on being forced forward suddenly penetrated into a cavity. The child's screams now ceased, but no urine escaped; all haemorrhage was, however, checked by the pressure of the instrument on the walls of the new duct. A few minutes after a caoutchouc bougie was substituted for the conductor, the eyes of which were filled with coagula. The penis was enveloped in compresses steeped in cold water; not more than an ounce and a half of blood escaped during the operation.

The child would not consent to take the breast, or to swallow milk and water, and it only drank a very small quantity of infusion of lime-flowers.

On the ensuing morning the little patient was without fever, and very little tumefaction was observed, no urine passed from the catheter, but the dressings were thoroughly saturated with a liquid of urinous odour, and during our examination a small stream of urine oozed without pain between the sound and the walls of the duct. The abdomen was neither swelled nor tender, very little drink had been taken, and the bladder could not be felt above the pubes. I did not remove the catheter before night, when a free discharge of urine took place. The next day the sound, which was easily reinserted, was replaced by a caoutchouc tube, which was left fifteen hours in the bladder. From this date the child's condition gradually improved; the penis soon recovered its natural aspect, the child took the breast, and relieved his bladder in a perfectly natural manner. No infiltration, or untoward symptom of any kind has occurred; seven weeks have now elapsed since the operation, the urethra remains pervious, and the infant is strong and healthy.

Several cases closely analogous to the above are recorded in a competitive thesis *on the malformation of the urethra in the human subject*, by Dr. F. Guyon, a Fellow of the School of Medicine of Paris.

ART. 104.—*On the Treatment of Acute Orchitis by puncturing the Testicle.*

By Mr. HENRY SMITH, Assistant-Surgeon to King's College Hospital.

(*Lancet*, August 6, 1864.)

"In July, 1863," says Mr. Smith, "a young man presented himself amongst the out-patients at King's College Hospital with

gonorrhœal orchitis in a very acute form. The pain was unusually severe ; and, on examining the organ, it appeared to me that suppuration had taken place, the sense of fluctuation being, as I thought, distinct. With a view of evacuating the pus, I took a bistoury, and made a free and deep incision into the supposed abscess ; but, to my astonishment and dismay, not a drop of matter escaped—only a little serum and blood. The tubes, however, of the testicle shot out, as it were, from the wound, forming a protrusion the size of a nut. Some pressure was applied by means of strips of plaster, and the patient was sent away.

"Two days afterwards the man presented himself ; but in a very different condition. He was quite free from pain, all the redness and most of the swelling had disappeared, and, on taking off the strapping, it was found that the protrusion of the tubes of the testis no longer existed.

"This case, which was somewhat annoying to me at the time, suggested some serious reflections in reference to the speedy relief which had resulted from a practice which in reality was the effect of an error of diagnosis on my part. Was the sudden relief here a mere accident?—or, if not, to what could it be due? The quantity of serum and blood abstracted was so small that the cessation of pain and diminution of swelling could hardly be due to this cause ; but it struck me forcibly that the free division of the fibrous tissue enveloping the body of the testis, and the consequent removal of tension from the organ, was the secret of the success, provided it was not a mere accident.

"Influenced by this reasoning, and by the result of this case, I determined to try the effect of puncturing the testis in similar cases ; and in the next case of acute orchitis which presented, I made a deep and free incision with a sharp narrow bistoury, emitting about half-a-teaspoonful of serum and several drachms of blood ; and no other treatment beyond a little of the common aperient mixture was supplied. The result here was as successful as in the former ; and as cases presented themselves, I adopted the same plan of treatment, reserving it, however, especially to those instances where the swelling and pain were very great. After the trial in a few cases, it was found that the success attending this practice was such as to lead me to adopt it as the usual treatment of acute orchitis ; and during the last twelve months I have probably treated in this way upwards of twenty cases, with results which have astonished both myself and those numerous pupils who have witnessed the practice.

"In nearly every case so treated—and I have purposely selected the most acute—the patient has experienced the most striking relief before he has left the out-patients' room ; and on the next visit, forty-eight hours afterwards, the contrast presented is so remarkable, that the superiority of this plan over the old-fashioned modes of treatment is at once impressed forcibly upon the minds of those even who would naturally be prejudiced against so apparently heroic a treatment. The speedy subsidence of all the acute symptoms is due entirely to the puncture of the swollen and inflamed organ, for I have taken especial care not to prescribe anything else except a

little of the common white mixture, or perhaps the use of the ordinary lead lotion, and this chiefly to please the patient.

"We all know what a terrible ordeal of violent remedies a patient with acute inflammation of the testicle has to undergo. In the first place, he is obliged to lie in bed for several days; a large number of leeches or the constant application of ice are necessary to relieve the pain; and at the same time the unfortunate wretch is compelled to undergo the process of severe purging, and continued nausea, by repeated doses of salts and tartar emetic, before any decided mitigation of his symptoms ensues; and two or three days mostly elapse before he recovers from the depressing influences of these several remedies. Lastly, the unfortunate organ has to be submitted to the tender mercies of a dresser, who, however skilful he may be, cannot help putting the owner of it to severe and prolonged torture whilst he is obeying the injunctions of his superior to 'strap testicle.'

"Now for all this I venture to submit the plan now proposed, and one which I should call a 'new' one; but it is venturing on dangerous ground to call anything new now-a-days. Moreover, my old assistant and our present house-surgeon, Mr. Richmond, informs me that when he was in Paris two years ago he saw the same method of treatment adopted there; but I never heard of it before I resorted to it, and the practice in my hands was entirely due to the accident I have related above.

"Of course several of my friends and pupils have urged objections against this plan of treatment, and suggested serious results, in the form of suppuration of the organ, impairment of its function, hernia testis and fistulous sinuses; but none of these have I witnessed. It is very natural and proper to make these objections, for we have always carefully avoided the possibility of a puncture of the testicle when using a trocar for paracentesis of the tunica vaginalis, and, indeed, I have witnessed violent suppuration of the testis speedily ensue from this accident; but it must be borne in mind that wounding of a healthy testicle with a large and blunt instrument like a trocar is a totally different thing from a careful incision made into the highly inflamed organ by a thin sharp blade."

ART. 105.—*A Case in which the External Iliac Artery was tied first on one side and then, after an Interval of some Months, on the other side also.*

By Mr. HENRY SMITH, Assistant-Surgeon to King's College Hospital.

(*Medical Circular*, August 25, 1864.)

Both operations were performed in King's College Hospital. The date of the second operation appears to have been in April or May last.

CASE.—This man is of strong constitution, and of that peculiar slender, "wiry" build indicative of strength; he is a carpenter by occupation, is a

hard worker at his trade, is very temperate in his habits ; he is about the middle age.

His history is as follows :—Some time since, he noticed a swelling in each groin, that on the right side being the larger ; and as it continued to increase in size, he consulted Mr. Smith, who found the tumour to be an aneurism of the common femoral artery.

Upon the true nature of the swelling being ascertained, the man was admitted into hospital under care of Mr. Smith, who, in accordance with the views of modern surgeons, applied pressure and maintained it for three or four weeks, without, however, effecting any cure of the aneurism, and only occasioning the man a great deal of pain, both by the actual pressure and by the sloughing of the integument consequent upon the pressure, and that too when it was applied with the utmost possible caution and with the nicest delicacy.

The pressure having failed, and the patient being anxious to have something done which would radically cure the affection and take away the constant jeopardy to his life, Mr. Smith thought it advisable to perform the ultimate operation, and accordingly put it into practice on the 11th of last August, by tying the external iliac artery. The result of this procedure was the happiest that could be desired. At the time of the performance of this first operation there was a tumour in the left groin, of the same nature as that on the right side, but of a much smaller size ; to this pressure was applied for two or three weeks, but as its only effect was pain and distress, it was discontinued, and the man was sent out to regain strength and general tone to the system, being kept, however, under constant surveillance, so as to watch the course of the tumour, and to be able to operate for its removal at the time most opportune.

As the swelling on the left side grew steadily larger, the patient was admitted into the hospital, but preferred to have the operation of tying the vessel performed, rather than again be submitted to the torture of compression. Mr. Smith, therefore, in acquiescence with the patient's own desires, because these were in accordance with what was proper, that is, with the laws of established surgery, proceeded to tie the external iliac vessel of the left side, in the same manner as he had done that of the right ; the only difference being as to the length of the incision, which, on the present occasion, was not carried so far upwards and outwards. The reason for thus limiting the line of incision was to obviate protrusion and descent of the intestine through the weakened walls of the abdomen, a condition which had already existed on the right side at the seat of the former operation. By having, however, a shorter incision, Mr. Smith hoped to prevent a hernia.

The patient being brought completely under the influence of chloroform, Mr. Smith beginning the incision somewhat internal to the centre of Poupart's ligament, carried it upwards and outwards towards the crest of the ilium for a distance of about four inches ; having cut through the integument, he then, layer after layer, divided most carefully each structure, affording protection from the knife by either his finger or the director. The lips of the wound were held apart by the fingers of assistants or by means of the retractor, Mr. Ferguson taking charge of the upper lip, so as to prevent any slipping down of the intestines upon the knife of the operator.

At length, after the most careful dissection and manipulation, the artery was reached ; and, lest there should be any mistake in taking it up, it was compressed by the finger to ascertain that the flow of blood was arrested throughout the entire limb by this compressing force. This expedient having been adopted, after a little further delicate dissection, necessary for

the passing of the aneurism needle easily between the vein and artery, these vessels were separated sufficiently to admit the needle between them, which was then done from the inner toward the outer side of the body—that is, from the mesial line outwards; and its point having been brought out on the outside of the external iliac vessel, the ligature, with which the eye of the instrument was traversed, being seized with the forceps and drawn out so as to have a single coil around the artery, this latter was tied.

Another ligature was thrown around the epigastric artery, such a procedure being considered prudent as a guard against secondary haemorrhage, which might otherwise possibly occur, owing to the contiguity of origin of the epigastric artery to the site of the ligature upon the external iliac. The wound in the abdominal walls was then united by the ordinary thread interrupted suture, over which strips of wetted lint were laid, these being in turn covered with a towel, and all being then secured by a calico bandage.

In his remarks after the completion of the operation, Mr. Smith adverted to the rarity of such a measure as that he had just put into force, namely, the tying of the external iliacs of both sides in the same patient.

(c) CONCERNING THE UPPER EXTREMITY.

ART. 106.—A New Procedure for the Reduction of Dislocation of the Head of the Humerus beneath the Coracoid Process.

By M. A. SALMON.

(*Journ. de Méd. et Chir. Prat.*, Nov. 1864.)

This method, in which the slowness of the procedure is the principal characteristic, is founded upon the precept laid down by Dupuytren, viz., That in order to overcome muscular resistance, it is necessary to proceed with the greatest gentleness and precaution, and to persuade the patient that the operator's intention is merely to examine the injured joint.

The patient should lie down, the side on which the dislocation exists extending beyond the bed, the injured arm being supported by an assistant. The surgeon grasps the fore-arm and hand, and very slowly raises the limb from the side, discontinuing his efforts when the slightest pain is complained of, and gently chafing the muscles of the shoulder. This period of the operation may occupy from ten minutes to a quarter of an hour.

During this interval the dislocated arm is gradually moved from the body, and finally raised straight up to the head, when reduction may be easily accomplished in the following manner:—

The raised arm is entrusted to an assistant, who supports it without effort in its new position, while the surgeon, placing himself on the inner side of the limb, fixes the scapula by applying his hands over the shoulder, and with both thumbs gently pushes back the head of the bone into the glenoid cavity, slight traction being at the same time exercised, if necessary, by the person who holds the arm.

The limb is restored to its natural position when the bone has been replaced. The operation is thus brought to a conclusion in a perfectly painless manner, and without the patient being even aware of the fact.

ART. 107.—*Suture of the Median Nerve.*

By M. LAUGIER.

(*Gaz. Hebdom. de Méd. et de Chir.*, Juillet 1 et 29, 1864.)

At a sitting of the Academy of Sciences, M. Laugier communicated to the society the results of the suture of the median nerve, which he had performed at the Hôtel Dieu, June 13th, in a case of severe wound of the left forearm. The radial and ulnar arteries, the palmaris longus and brevis, a portion of the flexor sublimis digitorum, and the median nerve, had been completely severed. There was abundant haemorrhage, in consequence of which the arteries had to be tied by M. Ledentu, the house-surgeon on duty, but the upper end of the median nerve could not be found in the bottom of the wound. The edges of the wound were next brought together by interrupted sutures, and the arm placed on a cushion, with the elbow flexed. The patient was seen by M. Laugier for the first time on Monday, June 13th. There had been no return of haemorrhage. Sensibility had disappeared in all the parts supplied by the median nerve, namely, the whole palmar surface of the thumb, index and middle fingers, and the inner aspect of the ring finger. It was simply diminished in the parts supplied by the radial nerve, and whilst it was normal over the dorsal aspect of the thumb and the first interosseous space, it was completely absent on a level with the index finger and the lower half of the dorsal surface of the middle finger. The movements of the opponens pollicis were abolished.

The wound being laid open and the arm extended, the lower end of the median was seen lying free above the annular ligament of the wrist, and measuring about $2\frac{1}{2}$ centimètres in length, but the upper extremity of the nerve could not be found. Chloroform was then administered, and an incision, about six centimètres long, was made through the middle of the upper flap, from the wound upwards, along the anterior aspect of the forearm. The flexor sublimis was divided longitudinally, and the trunk of the median exposed. The two extremities of the nerve were next drawn together and forced, by a reef-knot of a silk ligature carried by means of a nearly straight needle like those used in staphyloraphy, through the centre of each division of the nerve, at a distance of about twelve millimètres from its extremity. One of the ends of the ligature was cut off, the other brought out at the inner angle of the wound.

This operation is very rarely performed, and is almost restricted to experimental physiology. In practice, very serious theoretical objections have been raised against it, such as the fear of inducing acute pain, and fearful nervous accidents, convulsions, tetanus even.

M. Laugier, therefore watched over the case with care and anxiety. No marked pain followed the application of the ligature, no accident that could be ascribed to it. The traumatic fever, the swelling and redness of the forearm, were not greater than usual in such cases, whilst important results followed in the shape of a return of sensation and the power of motion.

On the very evening of the same day sensation returned a little; on the following day it increased remarkably, and the movements of the opponens pollicis became easy. This gradual improvement continued on the increase up to the Thursday, when it was easy however to ascertain that certain sensations were not perceived. On the Friday, the fourth day after the operation, pricking was obscurely felt, whilst differences of temperature were clearly perceived. The ligature came away on the twelfth day. Inflammation of the nerve followed upon this, as shown by shooting pains in the range of distribution of the median. All the parts supplied by the nerve were not affected, however, for no pain was felt in the ring finger. Numbness and gradual diminution of tactile sense also supervened in the two last phalanges of the index, the whole of the palmar surface of the thumb and middle finger, but sensibility remained unaffected along the anterior aspect of the first phalanx of the index finger, the outer half of the palm of the hand, and the outer surface of the ring finger. From the unimpaired sensation in the last spot, it might be argued that it was due to an anastomotic branch of the ulnar nerve, although there is little probability of its being able to supply the sixth collateral branch of the median. In that case, the inner surface of the middle finger should have remained sensible, unless it be supposed that the branch of the ulnar is exclusively destined to the ring finger.

A month after the operation sensation had returned perfectly, and the movements of opposition and circumduction of the thumb were executed with ease. On the tenth day, electro-muscular contractility, which, after traumatic lesions of motor nerves, disappears on the seventh or eighth day, was found normal by Dr. Duchenne (*de Boulogne*).

M. Laugier then mentioned a similar case which had occurred in M. Nélaton's practice, and in which that eminent surgeon had connected, by means of a wire suture, the cut ends of the median, after a portion of the nerve, two centimetres long, had been removed, together with a neuroma developed in its substance. Sensation and power of motion returned in the arm forty-three hours after the operation.

M. Laugier believes that it is not indifferent how the suture is applied. A thread, passed through a nerve, in a direction parallel to its fibres, injures few of them, whilst if it is placed at some distance from the cut ends, it does not act like a foreign body, and therefore does not tend to increase the inflammation. To sum up, the author lays down the following propositions:—1st. After tying the cut ends of a nerve, sensation and the power of motion return within very few hours, in the parts to which the nerve is distributed. 2nd. This return of the functions of the nerve progresses

rapidly. 3rd. The sense of touch and the power of motion are recovered before painful impressions and differences of temperature can be perceived. 4th. That the suture of a nerve, if the author's method be adopted, produces no special pain, nor is it necessarily followed by grave nervous symptoms, a fact, besides, which had been already noticed after the accidental ligature of some collateral nerves of arteries. 5th. That the suture of nerves of a certain size should be adopted in surgical practice, when their division affects the sensation and the power of motion of more or less extensive regions.

(D) CONCERNING THE INFERIOR EXTREMITY.

ART. 108.—*A Successful Case of Primary Amputation at the Hip-Joint.*

By MR. SPENCE, Surgeon to the Royal Infirmary,
Edinburgh, &c.

(*Edin. Med. Journ. July, 1864.*)

CASE.—On the 3rd of September, 1863, Robert Davidson, aged twelve years, was thrown from a truck which had been suddenly set in motion, and fell in front of it, both wheels passing obliquely over the upper part of the left thigh just below the pelvis. The accident happened at the Melrose station, and the boy was seen almost immediately by Dr. Clarkson, who adjusted the limb, and had him conveyed to his home at Newstead, about a mile distant. About two hours afterwards, Drs. Brown and Smith accompanied Dr. Clarkson to see the case. The boy had recovered in a great measure from the shock, but his pulse was still very weak. On examination, there was found a large contused flesh-wound at the upper and inner part of the left thigh, exposing the muscles, which were much torn and bruised, and allowing the finger to be passed deeply into the tissues of the limb. The femur at and below the trochanters was felt to be much shattered. The foot was cold, and pulsation in the popliteal and tibial arteries extremely weak, but there had been no great amount of blood lost.

As it appeared to the medical attendants that the boy's only chance of life was removal of the limb by amputation at the hip-joint, I was telegraphed for, and arrived at the patient's house about 9 P.M. On examination of the injury, the contused and lacerated state of the soft parts, the shattered condition of the femur, and the consideration of the great force by which the injury had been caused, left no doubt as to the practice to be pursued. I at once coincided in the opinion which his medical attendant had expressed, although, from the depressed state of the young patient, and the proximity of the injury to the trunk, the chances of success seemed very small.

The boy's parents having given their consent, I proceeded to perform the operation under circumstances not the most favourable. The room was small, and the only light procurable was from a small lamp on the mantelpiece, and two small candles held by a non-professional assistant; a wax taper I had brought with me was kept in reserve for exigencies. I had brought Lister's abdominal compressor, but as it could not be applied so as to command thoroughly the circulation, Dr. Smith took charge of compressing the common femoral on the brim of the pelvis, and I instructed one of the patient's friends how to command the bleeding from the posterior flap, by grasping it with one hand and pressing a large sponge upon its

surface. Dr. Brown took charge of the movements of the limb, whilst Dr. Clarkson administered the chloroform. When the boy was brought under its influence, I entered my knife between the trochanter major and the anterior superior spine of the ilium, and carrying it obliquely across the thigh, brought the point out a little above the tuberosity of the ischium, cutting a short anterior flap. Dr. Brown then rotated and depressed the limb, with the view of facilitating disarticulation; but owing to the shattered state of the femur, this movement did not produce the desired effect. Fortunately, however, this caused no great delay, for my knife had opened the joint in passing across the limb; and by grasping the upper broken fragment of the bone, so as to project the head, I completed the disarticulation, and cut as large a posterior flap as I could obtain from the uninjured parts. Some vessels on the posterior flap were first secured, and then the great vessels in the anterior flap—the vein being included in a ligature. I then removed some contused and doubtful looking portions of muscle. After all bleeding had been arrested, the flaps were brought together with sutures; and considering the nature of the parts from which the flaps were formed, they fitted tolerably well. The stump was then dressed, and the patient placed in bed, hot bottles applied, and some stimulus given, as he was very weak. He lost very little blood during the operation, as I ascertained by carefully collecting the blood from the stone floor, when it was found to amount to less than half a small teacupful; and altogether, with what was in the sponges, to about five ounces at most. After waiting till the little patient had completely rallied from the chloroform, and had got an opiate administered, I left him in the charge of Dr. Smith, who remained with him all night, and to whom I am indebted for the following report of the progress of the case:—

Examination of Limb after Removal.—On examination of the limb after removal, the femur, from the large trochanter downwards for about two inches, was found to be broken into numerous fragments, the ragged edges of which were embedded in the surrounding soft parts. The muscles and other structures were much bruised and torn, but there was no direct injury to the large vessels or nerves.

September 4th.—During the night, patient was very restless and delirious. Pulse 134, weak. A little brandy-and-water was administered at intervals, but was vomited. No reactionary haemorrhage of any consequence. *Vespere.*—Still restless, with delirium. Pulse 130. To have opiate with a few drops tinct. mur. ferr.

5th.—Restless night. Pulse 130, very weak and thready. Sunken aspect of countenance. Beef-tea, brandy, and milk given in small quantities frequently.

6th.—Rather better. Slept a little. Less delirium. Pulse 108, stronger. Wound has healthy appearance, except at the bruised part of the flap, which looks sloughy.

7th.—Pulse 108. Sloughing action extending slightly on posterior flap. Foetid sero-purulent discharge. Chloride of soda lotion. Opiate.

8th.—Pulse 90. No delirium. Line of demarcation formed, showing slough, rather exceeding a square inch in extent. Purulent discharge copious and less foetid.

9th.—All the stitches removed, and the flaps brought together as well as possible by strips of plaster. Pulse 100.

12.—Sloughs separated. An attack of diarrhoea. Slight delirium. Opiate.

14th.—Pulse 108. Diarrhoea less. Stump looking healthy. Diet for the most part milk, beef-tea, wine, with a little brandy.

16th.—Doing well.

20th.—Pulse has risen to 120. Had a restless night with delirium. Raw surface of stump covered with patches of whitish semi-translucent membrane of pretty tough consistence. Copious purulent discharge. Stimulants given in increased quantity.

21st.—Pulse 120. Occasional delirium. Wound has same appearance.

23rd.—Pulse 118. Wound has less of its diphtheritic-looking covering.

27th.—Stump looks healthy, and is cicatrizing round the edges. Dressed with sulphate of zinc lotion. Pulse still high, 116. Sleeps well. Appetite indifferent.

October 1st.—Pulse 110. Doing well.

4th.—Pulse 102.

From this time the recovery was slow but uninterrupted. The patient gained strength gradually, with the aid of tonics, wine, &c. The femoral ligature did not fall off till 4th November, two months after the operation, having evidently been retained for some time after its separation from the vessel by the granulations surrounding it. By the middle of December the stump was healed, with the exception of a mere spot. Two or three weeks later this also had closed, and the patient was able to move about with the aid of crutches.

Remarks.—I have thought this case would be interesting to the Society, because successful results in cases of amputation at the hip-joint are rare, and in primary amputations exceedingly so. In the last edition of "Cooper's Surgical Dictionary," it is stated that there have been six cases of primary amputation at the hip performed in Great Britain since 1838, and all of them proved fatal. In the Crimea fourteen amputations at the hip-joint were performed by British surgeons for injury, and not one recovered; and in the Punjab in 1848-49, three cases occurred, with the same unfavourable result. And, so far as I am aware, the case I have just detailed is the first successful primary amputation at the hip-joint in Scotland.

ART. 109.—*A Parallel between Excision of the Knee and Amputation above the Knee.*

By Mr. WILLIAM FERGUSON, Surgeon to King's College Hospital, &c.

(*Lancet*, July 16, 1864.)

Speaking on this subject, in one of the Lectures recently delivered at the College of Surgeons of England, on the Progress of Anatomy and Physiology during the present Century, Mr. Ferguson says:—

" But the true parallel, the true value of excision of the knee has never yet been drawn to its full extent, in as far as I am aware. Life—life and comfort may be considered as the highest and best result of surgical interference in a case of incurable disease of the knee-joint. Even yet we have not sufficient data on those heads. My own impression is that the question of life between amputation and excision will be pretty nearly balanced; indeed, I shall venture a step further, and say that if excision were to get all the subsequent comparative advantages willingly given to amputation, the hazard of one operation would be less than that of the other. If I am not mistaken, an idea prevails that excision of the elbow is in reality a safer operation than amputation of the arm; but such is not mine.

Few fatal cases of this excision have been recorded, but I have seen as many as to convince me that the mortality is probably as great as that of amputation above the elbow. Happily, such a result is rare in either instance.

" Whatever the mode of amputation, it must be acknowledged that neither skill, forethought, manipulative perfection, nor after-treatment, whether local or constitutional, can invariably avert some of the evils (less than death) which are known from ample experience to follow such an operation. The occasional evils of secondary haemorrhage, of unusual retraction, of scanty covering, of chronic sore therefrom, of caries or necrosis, of tender cicatrix, of neuroma on the great nerves, and of secondary operative interference, have all been in a manner ignored in this comparison. And yet how often have all men of experience seen cases of the kind. Neuroma is certainly rare in the lower limb; yet, allowing the most perfect result—allowing the stump to be above criticism, what is it after all? It is only a portion of thigh whereon to fix an artificial limb. If this cannot be done, the sufferer must ever after be dependent on crutch and stick.

" I beg it to be observed that I do not bring these features forward as objections to amputation through the thigh in cases where the operation may be deemed absolutely necessary. If that dire proceeding must be, patients must take their chance. Surgery can never entirely avert the occasional necessity for amputation, but in the case for which I now speak I maintain that she frequently can. And now let us see the case of excision. The foot and leg are left; the limb is shortened positively by the length of bone taken away—say from two inches to four. Allowing for the loss of growth in length before operation, and for arrest of growth after the operation—say five inches, say nine inches; allowing even more, the lower end of the thigh, the leg, ankle, and foot still remain. It is worthless in surgery to compare a bad stump with a bad lower limb after excision. Take the perfection of a stump, even in the estimation of the most critical, and even a medium limb after excision, and the comparison will not stand for an instant. With the best results, it seems absolutely absurd to compare the two.

" In the general comparisons hitherto made I take leave to set aside the evils of excision, as drawn or indicated by the authorities already named, as exceptions to the rule, and as being equalled in evil in most respects by those following amputation; but I say unhesitatingly that if the comparison is drawn between the perfect stump and the perfect result of excision, the comparison is just as unreasonable as that between an artificial limb and one of flesh and blood.

" Inequality in the length of the upper extremities is of little moment, but it is awkward, to say the least of it, in the lower, as we frequently observe after fractures and after disease of the hip or knee in early life. Such inequality may, in some instances, be attributed to bad treatment, but it often occurs despite the best skill in surgery. Yet who would in such a result say that the patient would have been better with the limb away by amputation? Who

does not think that when a person gets well of a diseased knee or hip, with shortening even to the extent of five or nine inches—no uncommon result,—he is yet somewhat fortunate—fortunate in not having had amputation performed through his thigh? We do see occasionally cases of great distortion of the lower limb after disease of the knee, but even these patients sometimes congratulate themselves on having the leg and foot. In some few such cases amputation in the thigh has actually been performed years after the so-called cure. Yet in such cases it is not our custom to lay blame on the treatment which may have been adopted, although I believe that it has often been highly defective. Whatever the amount of distortion after the cessation of disease in the knee-joint, I hold that, excepting very special cases, amputation is unjustifiable, as resection of the distorted knee is both safer and better. But I shall not press this point at present; let me rather again draw attention to the fact, that we never cry out against either Nature or the surgeon in cases of shortening and distortion of limbs after disease. Yet such defects are common. Whilst meditating these remarks, I have rarely known a day pass in taking one's ordinary rounds that I have not observed persons walking in the streets with shortened and distorted limbs after disease of the knee. Most of them have moved more nimbly, and with greater apparent security and comfort, than if on the artificial limb after amputation of the thigh. Yet shortening has in a manner become the bugbear of excision of the knee. I admit that it is a defect, but abstraction or excision and arrest of development are evils elsewhere as well as here. Again, I admit that in this locality disparity of size, particularly in length, is an awkward circumstance. Let us see, however, in what this consists? A difference of three, five, or nine inches! What is the contrast which has been drawn with this defective limb? It is with its neighbour! And here I imagine we hit upon the weakest of all the objections hitherto made to excision of the knee. The risk of loss of life, distortion, uselessness of that which is preserved, are all serious objections or blemishes to this proceeding. A short leg to a long one is, I again admit, a defect; but in this respect surgery no more fails than Nature does after disease. What, I ask, is the alternative for excision of the knee proposed by those who object to this operation? It is amputation through the thigh! I cannot allow that which might be an easy answer to the question,—Why perform an operation at all? Why not cure the disease, and thereby avoid amputation? That is a question of a totally different kind. I am not now discussing the question of amputation, or continued, and possibly other treatment to save the limb. It is the question between excision and amputation as regards the future condition of the limb. In ordinary amputation under such circumstances, half the thigh, or possibly two-thirds, may be left. The body is mutilated to nearly the entire extent of one extremity. Say what you may as to the quality of the stump, there is left a shortened femur, a shrivelled thigh; emphatically a stump. Even Samuel Johnson's explanation of the term gives an exalted idea of the noun-substantive which scarcely holds good with us: "The part of any solid body remaining after

the rest is taken away," is a flattering description of one of our stumps of the thigh. It is in reality with us barely more than a peg whereon to hang an artificial limb. In youth, in middle age, in advanced years, it never improves. It never can be more than a shortened bone, with shortened and shrivelled materials around ; and this even with the perfection of a stump. The defective results of excision I am disposed, in accordance with what I have said before, to class with defective stumps. In justice to the subject I now deal with, I take a fairly perfect result of excision. Whatever the shortness, that may readily be made up by a high-soled boot or shoe. There are left the lower end of the thigh, nearly the whole of the leg, the ankle, and foot ; the former two slightly damaged, the latter unscathed. The foot, leg, and thigh do as much as in the cases of distortion or shortening after disease ; and who, under such circumstances, would compare an artificial substitute to the limb of life?

" But I cannot leave the argument here. A well-healed stump never in reality improves, unless, possibly, it gets somewhat more callous, whilst often it gets more tender and irritable ; but the seemingly perfect result of excision at the end of six or twelve months (just when stumps are generally at the best) is no criterion of true perfection. If the limb is properly managed afterwards, it goes on improving for months—ay, for years. Without again discussing the question as to length, and without applying the remark to all, I can affirm, from ample experience in my own practice, that thigh, leg, and foot enlarge in bulk ; and, in particular, that with this change the leg and foot improve in muscular energy. It may to some be more impressive when I say that the calf of the leg shows again in increased muscularity and vigour. This observation has never yet, I believe, been dwelt upon by those who have written of the good qualities of limbs after excision ; but it is an important fact, and one which, in my opinion, goes far to balance that of shortening, which has been so eagerly put forward by writers previously referred to.

" I do not think that the value of the human foot has been sufficiently estimated by those who amputate, or even by some who advocate excision. It is certainly as wonderful in its mechanism, if not more so, as any of the organs of special sense ; and, without drawing a useless comparison between it and the hand, it is certainly in its entire state a thousand times more perfect, as part of the future support of the body, than the point of a thick stick, or any fabricated imitation of a foot. We see persons walking about with limbs shortened from various causes ; some with high-soled boots, some with apparatus of iron, some with a pin of wood to make up the proper length, each with a foot of tolerable dimensions and vigour. Whatever we may say as to grace or symmetry in these cases, we yet, under ordinary circumstances, consider that a fair compromise has been made with formidable disease. Even as an unfortunate maimed one who has suffered amputation through the thigh halts along, we may pity him as the victim of incurable disease, yet we claim his case as a bright illustration of the powers of surgery. The foot is rarely thought of, whether it has been swept away by ampu-

tation, or it be a portion of a shortened distorted limb. Yet I doubt if there is any substitute at all to be compared with it.

"In an ankylosed knee the foot is of great importance in progression. Besides the mechanical construction of the foot, its comparative size is of enormous value as a support; and what tissue of a stump in the thigh can compare with that of the sole and heel of the perfect foot?

"But I feel almost ashamed to carry this argument further. Persons out of our profession would think it strange to hear an argument in the College of Surgeons to show that the possession of a healthy foot is better than the absence of a foot; that a foot, leg, and lower third of the thigh, all free from disease, are better than nine inches only of a shrivelled thigh; that a nearly whole limb, two feet or two feet and a half long, is better than a shortened and shrivelled thigh which measures some nine or twelve inches from the hip! Yet such is the seeming argument to which I have been forced, for those who have opposed the operation of excision of the knee have almost invariably represented a good stump, as it is called, as superior to any limb preserved by such a proceeding. My own impression, however, is very different. I cannot say of a single instance coming under my own observation, that amputation would have been better. I am not here to say that excision must invariably be superior to amputation; but this I will say without hesitation, that however short the limb may be after this operation, the parts preserved, provided the original disease is cured and no special or great distortion be left, must be beyond measure superior to the best stump that can be made. I care not whether the limb may be five or nine inches shorter than the other, my impression is that were the femur so much diminished in length by cutting and by want of development that the foot on the damaged side should actually be above the level of the sound knee, its presence would ensure a better support than a stump of the thigh. I thus admit and take what may be considered an extreme case, such as has never yet been brought forward, and such as is likely to occur rarely, if ever. I do so to meet the exigencies raised by Messrs. Pemberton and Humphry, and particularly to meet the all-important question as to the propriety of performing this operation on young persons in whom the bones have not attained their full length. The reasoning—if such it may be called—on this question has been remarkable. It amounts to this: that as the limb does not grow after this operation in young persons in proportion to the rest of the body, particularly in proportion to the other limb, it should not be performed; for this want of growth, great or small, is an insurmountable objection. Yet, as I have shown, the shortening is likely to be as great after a tedious recovery extending over years from disease of the knee in early life. The question is not in such cases between excision and the cure of the disease: it is between excision and amputation. I take it for granted that, with few exceptions,—some of which I shall allude to ere long,—excision has been performed only in instances where amputation would otherwise have been the operation—instances where further attempts at cure were deemed likely to be useless."

ART. 110.—*Ankylosis of both Knee-Joints treated by Forceable Extension and the Subsequent Application of a Plaster Apparatus.*

By Dr. STABEL.

(*Gaz. Hebdom.*, Sept. 23, 1864.)

The patient was a young girl, aged fifteen, born of healthy parents. When only eight years old, she caught cold whilst at church, was brought home nearly insensible, and was attacked on the following day by acute inflammation of both knee-joints. Shortly afterwards, she became covered with a pustular eruption, which in some places gave rise to large suppurating abscesses. For the period of six years, she only took cod-liver oil, and from the violent pains she had in her lower limbs, kept them constantly flexed. The result was ankylosis of both knee-joints, for which she consulted Dr. Stabel, at the age of fifteen. Her legs were then completely atrophied, and about her sacrum, buttock, and knees were seen large cicatrices, the results of old abscesses. The ankylosis was of the spurious variety, so that the knee-joints were not immovable. The joint was normal on the left side, but on the right the slightly mobile patella was displaced downwards and outwards, its lower edge reaching beyond the head of the fibula. There were also partial dislocation backwards of the upper extremities of the tibia and fibula, and hypertrophy of the inner condyle of the femur, on the same side. The operation was performed June the 1st, 1859, under the influence of chloroform. The ankylosis was broken down by forcible extension, and the limb placed in a plaster apparatus. The left knee-joint was alone operated on on that day. There was some fever for a day or two after, and pain was complained of in the joint. In the following week the plaster apparatus was removed, and a hollow splint placed behind the limb. On the eleventh day, the right knee-joint was forcibly extended in its turn, and on this occasion no reaction ensued.

Within seven weeks after the first operation, the patient contrived to move about on crutches, and the muscles of the thighs soon became so developed that the upper portion of the splints had to be exchanged for larger ones. In the following January, she could stand without crutches, and could partially flex the joints voluntarily. She was ordered a course of the Kreuznach mineral waters, and her recovery was considered very satisfactory.

ART. 111.—*A Case of Spontaneous Gangrene of the Lower Extremity, accompanied by a rare form of Arterial Lesion.*

By Mr. SPENCE, Surgeon to the Royal Infirmary, Edinburgh.

(*Edin. Med. Journ.*, May, 1864.)

Commenting upon this case, Mr. Spence says:—"This form of arterial lesion—dissecting aneurism—is occasionally, though not

very frequently, met with in the aorta, but is of extremely rare occurrence in vessels such as the iliacs. Indeed I am not aware of any preparation showing such a lesion in any vessel smaller than the aorta, and in all the cases I have seen, the separation of the coats has been in the direction of the circulation ; but in this preparation the separation is towards the heart ; and I think this fact, taken in connexion with the history of the progress of the disease, will serve to determine what parts of the morbid appearance stand in relation to the gangrene as cause and effect. Looking at such a very evident source of obstruction as the peculiar lesion presents, we are apt to conclude that it was the originating cause of the gangrene. But viewing the morbid appearances, and especially the direction of the sanguineous dissection of the coats, in connexion with the progressive symptoms of the case, I think it much more probable that the originating cause was the more common one of coagulation occurring in atheromatous arteries, and that the lesion of the iliac probably coincided with the period of the exacerbation of irritative fever and excited circulation ; that the current of the excited circulation being obstructed beyond by the previous coagulation, caused the coats of the distended artery to yield at the diseased part, and the reflux current of the blood to force itself between the tissues of the artery, thus separating them in the backward direction towards the heart. If this view be correct, as I believe it to be, this case will point out a source of danger in reference to Brasdor's method of operating for aneurism, which has not been adverted to. Hitherto our great dread, in regard to that method, has been the risk of the rupture of the sac when the vessel was tied on its distal side, though that has never yet happened ; but it is clear the ligature in such a case would act like the spontaneous distal obstruction in the case recorded, and a similar lesion of the artery and a dissecting aneurism might follow."

CASE.—Mrs. Smith, aged seventy-nine, was admitted into the Royal Infirmary under my care, on the 18th of September, 1863, suffering from spontaneous gangrene of the left leg and foot. The history of her case, given me by her medical attendant, Dr. Wilson, was the following :—

"I first saw Mrs. S. on the 13th of August last. I found her in bed, complaining of severe pain in the leg and foot, which prevented her sleeping at night, and this she stated had begun some time previously. There was at this time no change in the appearance of the limb, and I did not then examine the state of its circulation. Her pulse at the wrist was rather frequent and feeble. She had been applying turpentine as an embrocation to the limb, which I advised her to discontinue. I prescribed opium pills, to allay pain and procure rest. The following day, when I visited her, I was informed that the pain had ceased, but that the limb was cold. On examination this proved to be the case, and no pulsation could be felt in any of the arteries of the leg. I carefully examined the state of the heart, but no abnormal murmur could be detected ; but her lips and face were rather blue. The limb was wrapped in wadding, opiates given to procure rest, and the patient's general strength supported. Gradually the limb became discoloured and shrivelled, and ultimately a line of demarcation formed below the knee. Under these circumstances, as the foctor was intense, and her general health not very much sunk, whilst the line of demarcation seemed

tolerably decided, she was sent to the Royal Infirmary, in the hope that amputation might probably be attempted."

On admission into hospital, I found her exhausted, and she had an anxious expression; her tongue was furred, and rather dry in the centre; the pulse was slow and felt firm, but this was evidently due to the atheromatous state of the arteries. The leg, though dry and shrivelled at the foot, had apparently died more rapidly above, as it presented more the appearance of humid gangrene. Immediately below the knee there was a deep line of demarcation around the limb, presenting a tolerably healthy granulating surface, covered with laudable pus. No pulsation could be felt, however, either in the femoral or external iliac arteries of that side, and I observed some slight mottling of the lower part of the thigh as if from superficial venous congestion, to which I drew attention, that any increase of it might be watched. At my next visit I found that the patient had suffered from nausea, and was very restless; there was heat of skin over the trunk and arms; and the pulse was more frequent and irregular. Her face, though flushed in the cheeks, had a more sunken appearance, and the tongue was dry. She was suffering from an accession of irritative fever. The thigh of the affected limb felt colder; the mottling from vascular congestion was much more marked; whilst the line of demarcation had an unhealthy, sloughy look, with purple edges. It was now quite evident that the gangrene was extending, and that little could be done beyond allaying pain, and relieving the patient from the foetor by applying Condy's fluid and charcoal. The symptoms gradually became worse, and the patient died on the fourth day after her admission into the hospital.

Leave having been obtained to examine the body, the arterial system generally was found to be affected with atheromatous and earthy degeneration. The vessels of the lower extremities, and the iliacs and lower part of the abdominal aorta were carefully dissected out, and reserved for further examination. The vessels of the leg and thigh affected by atheromatous disease were found plugged by coagula, as is generally seen in such cases; but in the left iliac artery, just at the point of bifurcation into external and internal, was seen a condition very rarely met with. At one part its internal and middle coats seemed to have given way, and the blood had forced itself between them and the external coat, so as to dissect or separate the tissues for some distance, coagulating between them, and throwing inwards the inner coats, so as to arrest the circulation at the affected part; and, what is still more unusual, the dissection of the coats was in the opposite direction to the natural current of the blood.

ART. 112.—*A Case in which a divided Tendo-Achillis was successfully united by Silver Wire.*

By Dr. G. L. SIMMONS, of Sacramento.

(*Pacific Med. and Surg. Journ.*, Jan, 1864; and *Amer. Quart. Journ. of Med. Sci.*, April, 1864.

CASE.—This case is that of a man in whom the tendo-Achillis was completely severed accidentally about an inch from its attachment. The upper edge of the cut tendon was retracted an inch and a quarter into its sheath. Dr. S. flexed the limb, drew down the retracted tendon by strong forceps, and united the cut end with a large-sized silver ligature; the leg was kept flexed for a few days with adhesive straps, after which the usual

slipper and dog-collar were used. In a few weeks the patient was able to walk in a high-heeled shoe with but little pain. Scarcely any stiffness resulted from the injury, and at the date of the report he could walk freely with the slightest perceptible halt. The "propriety of using silver wire in uniting tendons," Dr. S. says, "can hardly be questioned. In the above case the result was all that could be desired; and, although position alone might possibly have accomplished the same end in the same time, yet it is probable that the perfect approximation of the parts by the ligature assisted in defining the bond of union until it became strong. In this case, after the uniting mass was perfected, I removed the silver wire, as the play of the tendon caused the foreign body to slightly irritate the neighbouring tissues."

PART III.—MIDWIFERY.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

(A) CONCERNING PREGNANCY AND PARTURITION.

AET. 113.—*On some of the Signs of Early Pregnancy.*

By Dr. ROUTH, Physician to the Samaritan Free Hospital for Women and Children, &c.

(*Medical Times and Gazette*, August 20, 1864.)

In a paper read at the last annual meeting of the British Medical Association, Dr. Routh referred, first of all, to the signs of the first period of pregnancy from the date of conception to the fourth month. The symptoms then observed as diagnostic were, cessation of the menses, the purple colour of vagina, and velvety feel of the os uteri, the presence of kiestine in urine, enlargement of abdomen and breasts, and peculiar characters of areola. Only two of these, kiestine and an occasional modification of the follicles of areola, which he described, were at all certain. Auscultation was not supposed, except at the end of the first period, to assist. Naegele stated the placental souffle was heard certainly only at the fourth month, Rigby at the fifteenth and sixteenth week. Kennedy had, however, heard it as early as at twelfth, eleventh, and once at the tenth week, but his experience had not been confirmed by others.

The author thought by means of the vaginoscope, which he described, and which was in the main a single or double stethoscope with a vaginal speculum attached, that he had succeeded in diagnosing pregnancy by auscultation at a much earlier period.

The earliest sound heard was a general muffled murmur, once interrupted during the pulse beat. It was difficult to describe it. It was an intensely vesicular murmur, an attempt, as if it were, to produce the ordinary placenta souffle. This was heard from the sixth to the ninth week generally. Its pitch was higher when the placenta was attached near the os. The ordinary placental souffle was heard generally at the ninth week, but he had heard it distinctly at the seventh and eighth week, and once at the sixth. In non-pregnant women the vaginal pulse or a single cardiac sound was heard; also the sounds of the intestines, which, owing to the solidity of the uterus, were transmitted, and distinctly heard. These he had

never detected in the case of pregnant women where tumours (fibroid) existed in utero. These were heard, and besides the murmur was tubular, not vesicular, and often attended with a thrill.

The author instanced nine cases in proof: one of thirteen weeks and one day; one of thirteen weeks; one of thirteen, perhaps eleven weeks; one of eleven weeks and two days; one of twelve weeks and one day; one of ten weeks; one of nine weeks and two days; one of seven weeks, and one of six weeks and two days; in all of which early periods pregnancy had been made out by the vaginoscope.

ART. 114.—*Moveable Kidneys giving rise to Symptoms of Pregnancy.*

By Mr. EDWIN E. DAY, Assistant-Physician for Diseases of Women and Children, King's College Hospital.

(*Medical Times and Gazette*, July 9, 1864.)

The fact that kidneys are sometimes moveable is one on which some scepticism exists. On the Continent this condition has been well described by Professor Oppolzer, M. Rayer, and others, and in this country Dr. Hare has published some good lectures upon the subject.

To the general remark "Has it been found after death?" Mr. Durham has given a satisfactory answer by producing to the members of the Medico-Chirurgical Society the parts of a body in which the kidneys were abnormally situated and moveable. Professor Simpson mentions a case in which the kidney was surrounded by a special mesentery, thus giving it considerable range of motion.

There are three or four things with which moveable kidneys could be confounded:

1st. Fæcal masses in the colon.

2nd. Moveable spleen.

3rd. Tumours of the omentum or mesentery.

And lastly, some women have the power of throwing their psoas muscle into partial contraction, thus simulating this state of kidney.

Purgatives would decide the first point. The spleen would be larger than any kidney, and would, of course, be found on the left side, while it is commonly the right kidney which is moveable. Tumours of the mesentery or omentum would probably be confined to one side, and they would be anterior to the position of the kidney. The absence of the hilum would be a good assistance in determining the character of the tumour.

Dr. Priestley, in the *Medical Times and Gazette*, 1857, vol. i. p. 262, said he was convinced that some of the cases of spurious pregnancy which had come under his notice were cases of moveable kidney, but he did not give any cases; the following one may, therefore, be of interest in bearing out that opinion:—

CASE.—Mrs. W., aged thirty-seven, applied at King's College Hospital on June 3, 1864, under the following circumstances:—She was married, and

had had one child five years before. Since that time she had menstruated regularly till September last, when she was much frightened by the ceiling of her room falling in. After that she missed two periods and supposed herself pregnant, and was therefore surprised at finding that at the third period the catamenia returned, and have continued regular since. She still thought herself in the faintly way, as, to use her own words, "she had felt the movements of the child," but the nine months having passed without any change in her condition, she was anxious to have my opinion as to her state.

On making an abdominal examination, I found that the abdomen was tympanitic, and that there was no uterine tumour whatever, but in the right iliac fossa I felt a tumour, which slipped readily away from my hand, and upon closer examination was determined to be the right kidney. The hilum was readily felt, and the space over which the kidney could be moved was about three or four inches. On examining the left side, the kidney was also found moveable, but it was not so low down as the right. It was situated just below the level of the ribs, and could be moved over an area of two or three inches.

The movements which she had considered fatal were thus clearly to be attributed to these moveable kidneys.

In order to make the diagnosis more sure, I sent her into the hospital under the care of Dr. G. Johnson, who agreed in the opinion I had expressed.

ART. 115.—On the Application of Extreme Cold as an Anodyne in the Pain attendant on Parturition.

By Dr. J. M. GRANVILLE, Bristol.

(*Proceedings of the Obstetrical Society of London, May 4, 1864; and Dublin Medical Press, July 20, 1864.*)

An examination of the phenomena of labour, more especially in relation to the nature, duration, and intensity of the pain accompanying it, has led the author to the following conclusions:—

1st. The actual pain (as distinguished from the sensation of forcing) experienced by the parturient woman bears no definite proportion to the force and efficiency of what (for want of a better name) we term "the pains" of her labour.

2nd. The sensation of pain is not invariably synchronous with the occurrence of uterine contraction, the effects of the latter being perceptible to the accoucheur not unfrequently, during examination, before his patient experiences the former.

From these circumstances, taken in connexion with others well known to careful observers, it may be inferred that the pain attendant on labour is more directly related to the effects of uterine contraction (perhaps mechanical force operating on nervous tissue) than to the specific muscular act itself.

The reference of the pain to some region more or less remote from the contracting uterus or the dilating external passages (in which the seat of pain might have been supposed to be located) appears to support the last inference; whilst it clearly points to the conclusion that the pain attendant on labour is neuralgic in its character.

Acting on this theory, the author has tried the effect of extreme cold as a topical anodyne, and the result has been so constant that, after repeated experiment he feels confidence in bringing this new method of seeking to alleviate the pains of labour before the Society, and suggesting its adoption by the profession. The method is applicable in all cases, independent of the condition of the patient—cold being already in use as a powerful means of exciting the uterus to contraction in cases of extreme exhaustion. It is perfectly free from the dangers so inseparable from the most cautious use of anaesthetics. It is perfectly manageable, and may be safely confided to a nurse; and its *rationale* is in strict conformity with the well-known principles of physiology. It is not intended to remove, but to alleviate the pain attendant on parturition. It will be found that the power of the uterine efforts is generally increased, without any corresponding increase in the attendant suffering, the patient being not unfrequently under the impression that the pain has ceased when the presentation continues to evidence the continuance of the expulsive force. This has been very remarkable in some of the cases—the pains appearing to be shortened, when examination has proved them to be prolonged. The actual pain is sensibly diminished, the patients expressing themselves variously, but to the general effect that they are relieved of half their suffering.

The method of the application is of the greatest importance. The cold must be extreme, or it is of little, if any, service, and it must be persistent.

An apparatus consisting of a flat tin box, which is filled with a freezing mixture and applied to the seat of the pain, appears to answer very well.

ART. 116.—*A Modification of the Usual Mode of Extricating the After-birth.*

By M. SAUSSIER.

(*Journ. de Méd. et Chir. Prat.*, Oct. 1864.)

This modification is thus described:—

The cord, previously enveloped in a compress, should be seized close to the vulva with the forefinger and thumb, and, if necessary, with the middle finger of the right hand.

The left hand is laid over the hypogastric region. Immediately after the expulsion of the child, the abdomen becomes flaccid, depressible, and the uterus can be readily felt. The surgeon then applies the parted fingers of the left hand over the fundus of the womb, an operation which in general presents no difficulty.

Both hands being thus placed, the accoucheur closes the left, so as to press on the fundus uteri, and by gentle horizontal traction almost invariably removes the placenta with perfect ease.

If the after-birth does not immediately appear, the pressure on the womb and the tractive effort must be increased, and the organ at the same time brought slightly forward.

M. Saussier has for many years resorted to this very simple plan with entire success. In cases in which the old method had failed, on account of the resistance of the placenta, or the laceration of the cord, M. Saussier has always succeeded in promptly effecting delivery with perfect ease, the operation seldom requiring more than a few seconds.

Commenting upon the foregoing account, the editor of the *Journal of Practical Medicine and Surgery* says:—"We may confirm these assertions from our own recent experience. We have found expulsion of the after-birth so easy, so rapid under the influence of pressure thus directly applied to the fundus uteri, and the subsequent retraction of the viscera so perfect, that we have determined on never resorting to any other system. M. Saussier's plan spares the patient unpleasant and sometimes dangerous manipulations, and supplies us with the means of averting puerperal fever—a contingency which of late has become frequent, by promoting the occlusion of the gaping orifices of the uterine blood-vessels. M. Clozure, for the purpose of meeting the same indication, proposes the introduction of the hand into the uterus immediately after the expulsion of the foetus, and the forcible abduction of the placenta. But any practitioner who has given a trial to the two methods will certainly prefer that recommended by M. Saussier, of Troyes."

ART. 117.—*A New Variety of Dystocia.*

By M. PARISE, Prof. of Clin. Surgery at the School of Lille.

(*Journ. de Méd. et Chir. Prat.*, Sept. 1864.)

At a meeting of the Paris Academy of Medicine, held in August last, M. Parise reported the case of a woman for whom M. Depaül had been consulted, and who at her second confinement presented an unusual impediment to labour, in the shape of a partition which divided the lower part of the uterus into two separate cavities. The child's head was found on one side of the partition, and the feet on the other, the body lying across its superior margin. M. Parise succeeded in grasping the upper edge of the band, and divided it in its entire extent with a probe-pointed bistoury, an operation which was promptly followed by the expulsion of the foetus.

M. Parise conceives that in the present case the foetus had been simultaneously developed in the uterine cavity and in the parietes of the womb. This condition of course materially interferes with the progress of labour, and forms a new variety of dystocia. Most fortunately it can often be recognised in time, and effectually remedied, so as to save both mother and child. The following are its symptoms: a large rounded tumour occupying the upper part of the vagina, and apparently formed by one of the labia of the os uteri, within which portions of the foetus may be felt. The orifice of the womb is situated high up on one side of the mass, and assumes a crescent shape. This unnatural form of pregnancy might be mistaken for a tumour of one of the labia of the os tincæ, the symptoms

being the same in both cases, and the diagnosis must rest on the discovery of some part of the fetus within the mass. An error might more easily arise from the coincidence of uterine with interstitial gestation. The most ready means of discovering the real state of things consists in the introduction of one hand (the left if the tumour occupies the left side, and *vice versa*), and in ascertaining that the entire body of the fetus is included within the womb.

The only appropriate treatment consists in the division of the unnatural partition.

ART. 118.—*On the Breathing of Children before Birth.*

By Dr. BOEHR.

(*Mon. f. Geb.*, Dec. 1863; and *Med. Chir. Rev.*, Oct. 1864.)

Dr. Boehr has made fresh researches into the important question of the breathing of children before birth. Amongst his results the following deserve record:—He found that in the great majority of all prematurely-breathing children, and notwithstanding the most energetic attempts at respiration, there was no trace of air in the lungs. Dr. Boehr's cases embrace 10 in which the child lived on; 57 in which the child was dead born; and 10 in which the child was born dying. In all the cases examined after death, there was hyperæmia of the pectoral organs. Referring to the doctrine that petechial suggillations on the serous membranes are the most common and most constant appearances of asphyxia, he says, from positive observation, that these may be absent in cases of the most undoubted foetal asphyxia and drowning. His tables show that 18 times out of 75 dissections there were no petechial suggillations, and these were absent in cases where the placental circulation was interrupted from the most different causes.

ART. 119.—*Remarks on the Prevention of Hæmorrhage and After-pains, occasionally following Delivery of the Placenta.*

By Mr. GEORGE K. H. PATERSON, Balbeggie.

(*Edinburgh Medical Journal*, June, 1864.)

“Several years ago,” writes Mr. Paterson, “when actively engaged in lying-in practice, not a few multiparous women, ere they were delivered of the placenta, told me that as soon as delivered of it, they would have severe after-pains. And it was as true as they said; either sharp or violent uterine pains came speedily on, notwithstanding that the bandage was as usual passed round and well tightened; for this an anodyne was either given at once, or the patients were not interfered with for a day or two, then a purgative was ordered to open the bowels, when the pains either went off or gradually subsided. But, years ago, happening to observe that several of the multiparous women I was called to deliver, had, after the birth of the child and expulsion of the placenta, unusually large

or pendulous bellies, with more or less after-pains, before fastening the bandage I compressed the abdomen firmly with my open hands—one on each side of the linea alba—several times up and down and laterally, expelling at the same time a good deal of fluid blood and coagula, with decided relief of the pains, diminution of the size of the abdomen, and speedy contraction of the uterus; and a bandage being applied immediately afterwards, little farther attendance was required on my part, as the treatment seemed to hasten recovery considerably.

"In the lingering labours, too, of multiparous delicate women, and especially when there is found a flaccid state of the uterus, with more or less of haemorrhage and after-pains succeeding expulsion of the placenta, bi-manual compression has a striking and speedy effect, 1st, in expelling coagula; 2nd, in constricting the veins of the uterus; and 3rd, in inducing proper contraction of it.

"This practice, from its good effects, I have for years past continued; and but for it, I have not the slightest doubt whatever that haemorrhage and after-pains, or hour-glass contraction, attended by great exhaustion, if not loss of life, would have often occurred in the cases under my care ere other means had time to take effect. Indeed, now, in all my cases of labour, after complete delivery, I immediately compress the abdomen more or less as a reliable preventive of uterine irregularity, flooding and pain, previous to tightening the bandage, and have never to my recollection since I practised this method been called back on account of post-partum haemorrhage and violent pains, or hour-glass contraction.

"The readiness and freedom with which this preventive method of compressing the uterus through the parietes of the abdomen, when it is tumid or flaccid or torpid after labour, and its success in emptying the organ of irritating coagula and allaying agonizing pains, or in subduing these if present some time afterwards, as well as in checking haemorrhage, must commend the more general practice of it in preference to trusting chiefly to manipulation of the internal uterine surface; while no one can forget to take into account the greater risk by the latter method of increasing the haemorrhage, before it can be satisfactorily carried out, or other remedial means can be put in operation to cause the uterus to contract sufficiently. And in this readiness of application lies the true value of external compression when early made, as a preventive and effective power in post-partum haemorrhage, and of irregular contraction of the uterus after placental delivery in multiparous women."

ART. 120.—*On the Diet of Childbed.*

By Dr. GRAILY HEWITT, Physician to the British Lying-in Hospital, &c.

(*Lancet*, Sept. 3, 1864.)

In a clinical conference in midwifery held at St. Mary's Hospital, Dr. Graily Hewitt, after quoting from the writings of Drs. Churchill, Ramsbotham, and Tyler Smith, says:—

"From these quotations it is evident that the principle of practice recommended by these standard authorities is one of low diet from the first: Drs. Churchill and Ramsbotham ordering a low diet for as much as a week after labour has taken place; and Dr. Tyler Smith concurring in the principle of low diet as a rule, but admitting the exceptional necessity for deviation from this rule. The practice is, as I hope to show, wrong and unnatural. Nevertheless, the rules which I have mentioned to you are followed by a majority of practitioners. We have so grown up in the practice that it has hardly seemed to be extraordinary that a woman should be allowed little more than gruel, *ad nauseam*, for a week or more after her labour is over.

"Why is it that it has been considered necessary to place a woman recently delivered on a low diet? It was thought that the adoption of a low diet was likely to be the means of preventing puerperal accidents and diseases. This is the principle on which these rules are based. Is this principle true? Are known facts in consonance therewith? I believe the principle to be entirely wrong; I am quite sure that facts do not bear it out—nay, that they distinctly contradict it. Let us consider for a moment what is the condition of a woman directly after delivery. The nervous system is much agitated; she is often much exhausted; her muscular system has been exercised powerfully and to an unwonted extent; she has lost a certain quantity, in many cases a considerable quantity, of blood. The rational treatment of a patient presenting such symptoms would be a restorative one: it would involve (first) rest, and if possible sleep; and (secondly) the administration of such nourishment as would replace what has been lost; and it is obvious that the patient will require food in proportion to the amount of loss sustained. Further, it must not be forgotten that in many cases the patient, although not giving any obvious external sign of weakness or prostration, is nevertheless in a state very closely approaching to one of exhaustion; and this is particularly observed where the constitution has been undermined by rapidly succeeding pregnancies in women who are insufficiently fed and badly cared for. The rational treatment then, I would repeat, is to administer food such as will restore what has been lost; and by 'food' I understand whatever tends to support and maintain vital power—animal food especially, combined or not, according to circumstances, with liquid containing alcohol. So far as the condition of the patient immediately after labour is concerned, there would seem to be no reason for depriving her of such food and restoratives as would be administered under circumstances apart from the parturient state altogether, and with the view of alleviating similar symptoms.

"But, it is argued, the patient must be kept on a low diet in order to prevent mischief arising, and to ward off certain evils to which she is liable. A low diet will prevent, it is said, the occurrence of what is called 'inflammation.' Let us consider these various 'inflammatory' conditions liable to arise after parturition, with the view of ascertaining how far they are likely to be prevented, or the reverse, by the adoption of a low diet.

"1. *Milk fever*.—This is usually described as an affection which comes on about the third day, when the breasts begin to swell, the pulse rises, and there is a feverish heat of the skin, these symptoms subsiding in the course of twenty-four hours, more or less. From what we read in books, we should conclude that this is a common disorder; but the fact is that it is a very rare disease indeed, so much so that an eminent authority, M. Pajot, of Paris, almost doubts the existence of the affection. As bearing on this question, I may mention that out of the last fifty cases which have been under my care in the British Lying-in-Hospital, there were only two in which the symptoms present had any resemblance to those of 'milk fever.' This disorder is, you will perceive, ephemeral; no bad effects result from it. And now an important question arises—Would this disease be observed if the patient were well fed? My own experience has led me to the conclusion that milk fever is less likely to occur when the patient is well fed than under the opposite conditions. In the two cases which I have just mentioned as observed recently by myself there was present a markedly defective state of the nutritive functions, and both patients had been, prior to their admission into the hospital, very indifferently fed. I strongly suspect that 'milk fever' is in some cases connected with the practice, prevalent with some nurses, of not putting the child to the breast until one or two days after labour. This practice is one which I believe to be highly improper, and one calculated to lead to the production of sore nipples and milk abscess. On this point, however, I do not wish to enlarge at this moment. The point to which I wish particularly to call your attention is, that it is very questionable if a low diet tends in any degree to prevent the occurrence of milk fever.

"2. We come next to the more serious puerperal diseases—'*puerperal peritonitis*,' *puerperal fever*, *phlegmasia dolens*, &c. With respect to the pathology of these diseases, there is very much more to be said than can be compressed into the short space now at my disposal, and I can only state those conclusions respecting them which may, as I believe, be made a satisfactory basis for the application of therapeutics. It was formerly considered, and the idea is still prevalent to a wide extent, that the essence of these serious puerperal affections was 'inflammation.' Thus when, two or three days after labour, the patient began to complain of shivering, of pain over the uterine region, when the pulse became frequent, these symptoms were considered to indicate the presence of inflammation of the uterus or of the peritoneum. It is now known, however, although not sufficiently generally admitted, in the first place, that these symptoms frequently indicate the passage of poisonous material into the blood, really a form of pyæmia; and, in the second place, that while mischief of an 'inflammatory' kind may be set up in consequence of the introduction of such poison, or in consequence of violence sustained by the uterus during parturition, the best method of combating the inflammation is, not by employing remedies formerly considered anti-inflammatory, such as bleeding, antimony, mercury, administration of low diet, and the like, but by supporting the strength of the patient, and by exhibition of remedies of a soothing

and sustaining nature. So, again, in cases of puerperal fever; the condition actually present is a poisoning of the blood, attended with symptoms of extreme depression, in the prevention and treatment of which low diet and lowering agents of whatever kind are, in my opinion, noxious and injurious in the last degree. In phlegmasia dolens, another accident of the puerperal state, the essence of the disease being erroneously considered to be 'inflammation,' it was supposed that a low diet would tend to prevent such inflammation. The word 'inflammation' has much to answer for in respect to the injurious influences it has exercised on the treatment of puerperal diseases. It is responsible for the low-diet system which has so largely prevailed in the lying-in-room—a system which, by weakening the patient, has rendered her liable to become a prey to the poisonous influences by which she may be surrounded, and has induced a mode of treating puerperal diseases calculated to neutralize and negative the efforts Nature will always make to overpower and throw out the subtle agent creating mischief within. In the prevention of puerperal fever, the first thing to do is to prevent contact with septic agencies from without; the second, to secure the patient from the operation of septic agencies within. The latter indication is best fulfilled by securing early, good, and permanent contraction of the uterus. A relaxed uterus readily becomes the medium of absorption from the inner surface of the organ through the open extremities of its torn vessels. Perfect contraction of the uterus is, I believe, an almost complete safeguard against introduction of septic matter into the system, and contraction of this kind is best maintained by keeping up the vital powers of the patient, which can only be done by taking care that she is well nourished. Defective contraction of the uterus I have invariably observed to be present at the outset of an attack of puerperal fever.

"Modern pathological research has removed phlegmasia dolens from amongst the affections requiring an antiphlogistic treatment and prophylaxis. The substance which fills the hardened vein was formerly believed to be the product of inflammation, but we now know that it results simply from coagulation of the blood. The blood coagulates in the veins; the clot may soften, and become converted into a soft, puriform material, which, though looking like pus, is only broken-down fibrin. Phlegmasia dolens may occur in men as well as in women who have not had children, and it is not unfrequently noticed in cases of phthisis. Phthisis is, as we all know, not an inflammatory disease, its distinguishing element being defective nutritive power. It has been shown by Professor Humphry, of Cambridge, that this tendency to coagulation in the veins, apart from puerperal influences, is associated with a depressed condition of the vital powers, and he has offered abundant clinical evidence of the correctness of this statement. Now, in the case of a woman recently delivered, a depressed condition of the vital powers is very far from uncommon. If the uterus does not contract, an unusual quantity of blood remains in its vessels, and there coagulates. The coagulum spreads upwards by extension, and when it reaches the common iliac vein the circulation in the external iliac vein may be-

come stopped at any moment. Undue loss of blood during or after parturition necessarily depresses the system, and facilitates coagulation in the uterine veins, a tendency still further increased by the circumstance that the uterus in such cases does not contract well. That phlegmasia dolens is more often observed after parturition, in cases where much blood has been lost, is a matter of observation ; that it has been noticed to occur very frequently in cases where the vital powers have been inadequately sustained by nutritive material will become also evident to those who will take the trouble to inquire into the matter. The evidence to be collected, pathological as well as clinical, is all in favour of the proposition that by a generous diet will the tendency to phlegmasia dolens—supposing it to exist—be likely to be counteracted.

" If, for the sake of argument, we admit that these puerperal accidents are inflammatory, the utility of a low diet cannot be maintained in face of the great alteration which has come over the professional mind in reference to the treatment of inflammation. The practice of bleeding has very largely gone out ; mercury and antimony are far less relied on than formerly. There is certainly much doubt as to their efficacy in these cases. The absolute dietary formerly insisted on has equally fallen into disfavour.

" It may be urged that I am arguing on theoretical grounds ; but I can state, as the result of very careful personal observation, that the conclusions I have enumerated as to the bad effects of the low-diet system in the prevention and treatment of the puerperal diseases alluded to are amply borne out by the facts in my possession. I have also—and this is perhaps more to the point—abundant evidence of the most practical kind of the value of a generous sustaining and supporting diet and regimen, both in cutting short puerperal mischief of the worst kind, and in preventing its occurrence under circumstances most threatening to the patient. What I have seen of puerperal fever and allied disorders has, indeed, induced me to regard with the utmost horror all remedies of a depressing, lowering character. In the treatment of these affections, large quantities of food and brandy, or an equivalent, I have employed most successfully. It is rational to suppose, and it is consistent with my experience, that this gives a clue to the prophylaxis of these diseases. I say nothing of cleanliness, ventilation, separation from contagious influences, &c. : the necessities for these it must be superfluous for me to expatiate upon.

" 3. *Puerperal mania* is another affection here to be alluded to in connexion with the subject of the diet of child-bed. It will be sufficient, perhaps, for me to state in reference to this disease, that a generous diet, with opium in large quantities, and absolute rest, mental and bodily, form the essential elements in the treatment. Here, also, the clue to the prophylaxis is offered by the treatment. The disease generally results from the combined action of excitement and weakness, however induced.

" 4. *Sudden death during the puerperal state*.—This is an occurrence rare, but of great interest. In the cases which have been investigated, the accident has been found to be connected with coagulation

in the veins and obstruction to the circulation produced by the coagula in question. This form of death is one of the results of what is now known as 'embolism.' What I have already said in reference to the circumstances which lead to coagulation within the veins after parturition will enable you to understand why it is to be expected that a low diet will favour the occurrence of this lamentable accident. Apparently the best possible preparation for such a disaster would be to keep the patient on a very low diet, to prevent all motion of the body, thus favouring stagnation of the blood in the great vessels, at the same time neglecting to take precautions to insure uterine contraction.

"5. *Protracted convalescence.*—This is, if not a disease, certainly a great evil. That the observance of a rigidly low diet during the period of lying-in does tend to render the convalescence protracted does not admit of a doubt. This has been forcibly stated by Dr. Oldham in his address to the Obstetrical Society, to which I have already alluded. 'The precepts laid down in some of the midwifery books,' says Dr. Oldham, 'for the management of the puerperal state, steadily induce a debility in the first fortnight which requires a drawling convalescence in the second fortnight to overcome. . . . From first to last elements of weakness and nervous disorder are introduced, and the very diseases are invited which they were designed to remove.'

"We have now considered *seriatim* the chief of the evils which have to be prevented or encountered during child-bed, and I think it has been rendered evident that the supposition that a system of a low diet is calculated to remedy and prevent these diseases is a mistake. The actual practice of those best informed on these subjects has of late years undergone a very marked change. Dr. Oldham is not alone in his practice of supplying the puerperal patient with food of the best kind, and in good quantity, from the very moment of her delivery. That the time has come for the adoption, by the profession at large, of a more rational principle of treatment cannot be questioned. And now let me state that the views expressed by the illustrious Denman on the subject of the diet of child-bed are in perfect agreement with those for which I have been contending—namely, the impropriety of depriving the puerperal patient of her ordinary food; but his precepts on this point seem to have entirely passed out of professional recollection. Denman says: 'After seeing and considering much practice, and trying various methods, not only immediately after delivery, but through the course of child-bed, I am fully persuaded that, laying aside all refined speculations, those patients will fare the best and recover most certainly and speedily by whom the least change from their former habits is made. . . . The general principle of making as little change as possible from their former habits and customs either in diet or in any other respect, will best satisfy the expectations of the medical attendant' (vol. ii. p. 449.) What I now advocate is a return to these principles of practice. With reference to the particular diet suitable in different cases, it is unnecessary that I should enter into any lengthened detail. It is obvious that the quantity of

food must be proportioned to the requirements of the patient: one will require meat once, another two or three times in the day. As a general rule, Denman's advice, to make little change in the ordinary diet, should be followed; when, however, the labour has been severe or long, when an unusual quantity of blood has been lost, or where the constitution has been weakened by previous illness of any kind, stimulants are, in my opinion, almost imperatively required, unless the patient be able to take animal food, milk, eggs, &c., easily and in good quantity. The exhaustion produced by the labour frequently destroys for a short time the appetite for solid food, and at this period it is necessary to administer nutritious liquid food—milk, soup, beef-tea, eggs beaten up with wine or brandy (and a sufficient quantity of the latter) in order that ground may not be lost."

ART. 121.—*On the Results of Recent Improvements in Operative Midwifery in diminishing the number of Cases requiring Embryotomy.*

By Dr. ANDREW INGLIS.

(*Dublin Medical Press*, Sept. 21, 1864.)

In this paper, which was read before the Obstetrical Society of Edinburgh, Dr. Inglis proposes to state the effects which the more recent improvements in the method of effecting delivery in cases of contracted pelvis, must have had on the proportion between the favourable and unfavourable results of craniotomy, and then, by reference to the previous statistics of the operation, to indicate what its value now appears to be. The three changes which seem to act most powerfully in producing such an alteration are—the use of chloroform, the improvements in the form and in the manuer of application of forceps, and the employment of podalic version.

"The effects of the use of chloroform in increasing the death-rate of craniotomy must be considerable, for though it renders the operation somewhat easier, and perhaps even safer, still this is not the case to such an extent as can materially affect the statistics of the operation; while, on the other hand, its exhibition, by permitting the substitution, in the milder cases of contraction, of a less severe method of treatment, deprives craniotomy of a number of the very cases which formerly furnished the greater part of its recoveries. For in many cases where, without chloroform, craniotomy would have been resorted to, the use of that agent, by procuring an early dilation of the soft parts, gives an opportunity for delivery by forceps, or, by bringing about the same relaxation, accompanied by the suspension of uterine action, enables us to extract by turning.

"While chloroform has thus been doing much to facilitate the use of the forceps, great improvements have also been made in their form and in the manner of using them. This has not been suddenly effected; but the profession has, in course of time, gradually become better acquainted with the shape of the pelvis, and with many other collateral matters, which has not only enabled it to use the old

form of instruments more efficiently, but has also occasioned successive changes in their shape. The result of these improvements has been not only to increase the safety of the operation in the cases to which their use was formerly confined, but, in addition, to extend their application to many of the less severe cases of contraction then considered to require craniotomy, thus again tending to diminish the favourable results of craniotomy in those cases to which its application is thereby chiefly confined.

"Podalic version, which has long been employed for other purposes, and even suggested at an earlier period in the cases now under consideration, has only comparatively recently been much used in labour rendered difficult by pelvic distortion. Not only is it, however, now used in many cases in which forceps would otherwise have been employed, but it has been performed with success as regards both mother and child, in cases where the deformity was too great to admit of their use.

"It was some time ago shown very clearly by Dr. Simpson, that in many cases of pelvic distortion the child was born alive when the lower extremities presented, while in the other labours of the same patients none were saved when the head presented—delivery through the natural passages having been impossible without craniotomy. He therefore proposed in such cases always to turn and extract by the feet, and the value of this method of practice has been fully established by its results; and even when the child cannot be extracted alive by this operation, but dies in the course of it, or in consequence of additional operative procedure being found requisite, the danger to the mother is inconsiderable compared with that entailed by instrumental delivery in the natural position of the child. That the use of version has therefore, in the same way as the improvements relating to forceps, and to an even greater extent, lowered our estimate of the value of craniotomy, will be at once admitted; though at the same time it may be remarked, that if it were prefaced, when possible, by turning, much might be done to diminish the mortality in the cases where it is still performed.

"But, in estimating the effect of all these improvements in reducing the number of cases formerly held to require craniotomy, we must especially take into account that it is only to the milder cases of pelvic distortion they are applicable; and, as the occurrence of the more formidable cases of contraction becomes rarer in a ratio which increases very rapidly with its increase in degree, a proportionally rapid diminution of the number of cases still requiring craniotomy must be a necessary consequence; while the unfavourable circumstances under which it has to be performed must be accompanied by a corresponding increase in the ratio of mortality attendant upon it.

"To enable us to determine precisely the present value of the operation of craniotomy, accurate statistics would be required; but as these do not exist, I must instead attempt an approximation to the present death-rate by inferring from previously existing statistics the probable results of what has now been stated. The previous statistics are well known, and the mortality deducible therefrom is

one in four or one in five; therefore, if what has already been stated as to the effects of improvement in practice be correct, it will be quite safe to assume the present mortality to be very considerably greater than one in four or five; or probably one in three would not be a rash assumption.

"The next step towards the attainment of a proper appreciation of the merits of the operation seems to be the comparison of its results with those of other operations applicable to the same class of cases. As craniotomy is now confined by the best authorities to cases in which delivery cannot be effected by means of forceps or turning alone, and as the mortality of these operations is hardly appreciable, it is evident that a comparison between them and craniotomy may be left out of consideration, as affording no criterion for such an estimate as that at which we wish to arrive; but with Cæsarean section the case is different, for although at present it is true that that operation (in consequence apparently of reliance on old statistics) is considered justifiable only where craniotomy cannot be performed with success, as regards extraction, the difference in the mortality is, it is believed, by no means certainly in favour of craniotomy.

"With regard to the Cæsarean operation, we are now in a much better position to obtain success than we were a few years ago; and I believe there is a paper by M. Duseilly on the operation, in which he has collected the statistics of all the cases performed since 1858, and shows that, where the operation has been performed with ordinary care, at the proper time for interference, the results have been about seventy-five per cent. of recoveries—that is to say, one death in four, not being much more than the result of craniotomy as performed under the old régime, and consequently in all probability more favourable than those of the same operation, limited in its application, as it may now be said to be, to a small number of cases of a very unfavourable description. There is another point also to be noticed in favour of Cæsarean section. In this country we have improved rapidly of late in the performance of ovariotomy—an operation very similar in many respects, but having additional complications of a serious character; and, if we can produce by it such favourable results as one death in four, where, by enormous incisions and dissections we remove from the abdomen a part of the frame itself, and are under the necessity of leaving behind much that must separate by suppuration, how much more success ought we to expect in removing from the same cavity a foreign body, without being obliged to make such extensive wounds, and not necessarily having to leave behind anything to slough away. Besides, if we examine the individual reports of cases of Cæsarean section, we shall find, on the one hand, that in many of the fatal ones, setting aside the question how long the patient had been in labour, the operation had been so ill done that death could not fail to ensue; and on the other hand, that among the recoveries there were some where it had been so badly done as almost to put a favourable result out of the question, leading us to infer that on both these accounts, if due care had been taken, a much higher success might have been attained.

" Since writing the above, I have heard that Dr. Tyler Smith has written a paper advocating the abolition of craniotomy, but I am sorry I have not yet had an opportunity of perusing it, so as to consider his objections, many of which must be the same as those now brought forward; but I am glad to hear that I do not stand alone in questioning the correctness of the value at present usually put upon the operation.

" From the foregoing it will be apparent that unless something considerable can be done to render craniotomy a less fatal operation than it is at present, Cæsarean section must prove, at least, a formidable rival. I have already stated my belief that a good deal might be done in this direction for a certain class of the cases held to require craniotomy, by the use of turning as a preliminary to the operation; but as, after deducting these, there would still be left a great many cases, with a mortality certainly even greater in proportion than that of the whole number in which craniotomy is at present performed, it would become only the more incumbent on us to see if we should really be justified in preferring it to Cæsarean section for the cases where turning cannot be effected on account of the narrowness of the pelvis alone.

" The best form in which I can express my views as to the value to be assigned to craniotomy, seems to be the suggestion of rules for its employment; but before attempting this it may be as well to consider the nature of the rules at present generally accepted.

" At present we have a tolerably sufficient knowledge of the indications demanding interference of some kind, but the limits beyond which the different operations ought not to be attempted are so variously laid down as to give rise to much confusion. The principal guide given for the treatment of such cases is the minimum number of inches in the different diameters of the pelvis through which a full-sized child can be extracted—in one case by means not necessarily destructive to it, and in another by means in principle involving its destruction. This guide is obviously unsatisfactory, for it is hardly possible to get any two men to agree as to the measurement in inches of the brim of the pelvis in a patient; and, moreover, there are great differences of opinion as to the exact measurement required to decide which operation ought to be attempted in any given case. Then these measurements are laid down without reference to the probable size of the child, which, as we know, may vary in weight from six to twelve, or even fourteen pounds, at the full time. Besides, its consistency may also alter the prospect of the ease most seriously. We have also another source of dissatisfaction in dealing with this operation—viz., the possibility, if the result be successful, of a doubt remaining as to whether a milder method of interference might not have been at least as safe; and we never can be so sure after such a success, as we are in most other surgical operations, that we have adopted the best possible means. I shall now merely, in conclusion, give the rules to which I have been led by personal observation of cases of labour rendered difficult by contraction of the pelvis, combined with the study of published reports of individual cases of the kind.

"1st. In all cases where the indications for interference are sufficient, the forceps should be applied, provided there is a reasonable chance of their being successful without injury to the mother.

"2nd. Should forceps be found unsuitable, recourse should be had to version, even though the extraction of the head with instruments be afterwards required.

"3rd. Should the pelvis admit of turning, but be too small to allow the extraction of the body, we ought seriously to consider the advisability of Cæsarean section.

"4th. Should the pelvis be too small to admit of turning, the arguments in favour of Cæsarean section must be still stronger.

"I have divided the cases included in the last two rules into two sets, one where turning is possible, and the other where it is impossible, because I believe that such a means of distinction is better than the old rule by measurement in inches; and I have not insisted on Cæsarean section in the former of these cases, where turning was found possible, because I have heard of the head having been returned and craniotomy having been performed on it *in situ*, but with a fatal result, and it is possible (though improbable) that some successful precedent may have given rise to such a method of practice. The whole of these remarks on craniotomy have been made with reference to the size of the pelvis alone; and though I have said much in disparagement of the operation, still I admit that there are exceptional cases where, from the state of the uterus or some other cause, it appears to be inevitable.

"There is another point I have not taken notice of—viz., the value of the life of the child. I have not done so because, though I think it might be an additional reason in favour of Cæsarean section, still it cannot at any time be compared with that of the mother, so as to materially influence our decision, where there is a more reasonable hope of preserving the latter by adopting other means; but wherever in any individual case we come to the conclusion that Cæsarean section gives to the mother a chance of recovery equal to that afforded by any other means that can be adopted, then the additional chance of saving the child becomes most important in determining our choice."

Dr. Inglis states that since writing the paper he has had a case where craniotomy had to be performed. The patient, a young woman, was in labour with her first child. The head was prevented entering the pelvis by a sharp anterior curvature in the lumbar vertebrae. After some delay, with no advance being made, Dr. Weir saw the case with him. They applied Professor Simpson's cephalotribe, but owing to the head being so high up, the instrument was found to be too short to take a proper hold. They then had recourse to the ordinary craniotomy instruments, and with them they were able to extract a large portion of the occipital bone, with part of the os magnum. With some difficulty they finally succeeded in extracting the child. The woman sank, and died on the third day.

ART. 122.—*Statistics of Queen Charlotte's Lying-in Hospital.*

By Dr. G. B. BRODIE, Asst.-Physician to Queen Charlotte's
Lying-in Hospital.

(*Proceedings of the Roy. Med.-Chir. Soc.*, May 20, 1864.)

The author commenced his paper by observing that in consequence of a notice having appeared in one of the Medical Journals for 1862 relative to the mortality observed in the lying-in hospitals of London, and in Queen Charlotte's Hospital in particular, the present paper was commenced to give the desired information, and to show that all the deaths which took place in the hospital were not from puerperal causes. For the last thirty-six years, from 1828 to 1863 inclusive, the registers have been carefully kept, but no detailed accounts published; during this period 7736 patients (producing 7824 children), were delivered in the hospital; of this number 3611 were single women, who are admitted if it is their first pregnancy, and of these 126 died; the remaining 4125 were married women, of whom 72 died. The great mortality existing amongst unmarried women on their passage through the puerperal state was then touched upon, and some reasons were given to account for this fact, of which the separation from their friends, their living in seclusion for some months previous to their admission into the hospital, exposure to atmospheric changes, and semi-starvation, all tending to depress the mental and bodily powers, were the principal. Some reference was also made to the married patients, who are often admitted into the hospital in labour very shortly after they are married, and a case in illustration of this which fell under the author's notice was given, in which the patient walked from the church to the hospital, registered her letter as a married woman, and was admitted in labour within three days from that date; the mortality amongst the married patients would, therefore, be considerably diminished if it were not for the fact that so many are exposed during the period of gestation to the same depressing causes as the single patients. The first table (Table I.) showed the number of deliveries, and number of deaths, with the death-rate for every year since 1828. The number of women delivered at their own homes annually was also given, but of these, there being no trustworthy records, no further notice was taken. A detailed investigation into the cause of death in each year was then entered into, the results of which were given in Table II.; for four years the hospital presented a clean bill of health. Puerperal fever (under which denomination its many varieties were necessarily included) was found to have been present in 123 cases, no less than 99 of which were in primiparae; 75 out of the 123 cases were among the single women; the death-rate from this cause was 1.57. The various periods of the year in which these cases of puerperal fever occurred was given in Table III., the result of which went to show that the healthiest months—or the months in which the least number of deaths happened—were October and August; after these the following order was observed:—November, April; January, July, Sep-

tember, December; May, June; February, and March. For eight years the wards were free from puerperal fever. Mania was found to have been present in 16 instances, 15 of these were in primiparæ, and no fewer than eleven were single women; in 12 out of the 16 cases, the age of the patient was between twenty and twenty-four. The remaining cases referable to puerperal causes, as post-partum haemorrhage, ruptured uterus, coma, exhaustion, &c., were 22, making, with the puerperal fever and mania, a total of 161. Of the non-puerperal causes, phthisis and diarrhoea were the principal; pneumonia and some of the eruptive fevers being also present; making altogether a total number of 41, which with the 161 deaths from puerperal causes, gives the original number of 202. Another table gave the total number of cases admitted in each month during the thirty-six years, with the corresponding number of deaths from all causes during the same period, with a result very similar to that obtained when puerperal fever alone was observed. The death-rate of the hospital was then entered into, which was shown to be, for the married patients, 1·84; for the single patients, 3·48; for both together, 2·6. The death-rate has varied considerably at different times; it was at its highest in 1849—the year of the cholera—and has varied from that to *nil*; in all cases but one, in which the death-rate was unusually high, the cause was found in the fact that the deaths were principally amongst the single women; for instance, in the most fatal year, 1849, the deaths of the single patients were just double the married; in 1850, there were 8 single and no married; in 1860, whereas 14 single women died, there were but 4 deaths amongst the married patients; in this lies the cause of the comparatively high mortality of Queen Charlotte's Hospital. In another table was given the death-rate observed at the "Rotunda Hospital," Dublin: it was obtained by comparing the deaths with the number of admissions during the last thirty-six years, and was found to be 1·45 per cent. At the "British Lying-in Hospital" the death-rate from 1849 to 1861 inclusive, was '69, the number of admissions was very small, hardly exceeding 1550 patients. Both of these hospitals, however, profess only to admit married women. The author went on to remark that, in his opinion, the high death-rate of the "Queen Charlotte's Hospital" was in a great measure due to the fact mentioned at the commencement of the paper, that so many of the married women are exposed during the period of gestation to many of the depressing moral agencies of a single life. A comparison was then drawn between the mortality of a lying-in hospital with patients delivered at their own homes; the out-door midwifery department of St. George's Hospital (restricted to married women) was taken as an example; the number of patients delivered was 2800, the number of deaths 11 (one of these was a case of abortion, and happened in the hospital); the death-rate, therefore, was 3·8, very favourable when compared with 1·84 or 1·45, and this, notwithstanding the filth and poverty in which they live. The author concluded this portion of the paper with a brief account of the foundation of the hospital, and the following sketch of the ventilation and plan:—The hospital contains two floors for the reception of patients, one for married and one for single women. On

each floor are six wards, containing three beds each, in which the patients are delivered, with an average of 1000 cubic feet space to each patient; but as each ward has not always its complement of three patients, the actual quantity of cubic space to each bed is sometimes increased. On each floor also is one convalescent ward, containing six beds; the walls of the wards are made of Parian cement, painted and varnished so as to enable them to be thoroughly well washed; throughout the building runs a corridor 84 feet long by 7 feet wide, having a window at each end, and a large staircase opening at its centre, in the roof of which is a ventilating opening, protected by a cowl; the whole of the Hospital is thus completely ventilated. Each individual ward is ventilated by means of an opening in a shaft that is carried to the top of the building, an Arnott's ventilator in the chimney, and an opening (that can be closed) over the door, communicating with the corridor; the windows are on the plan adopted at St. George's and the Middlesex Hospitals, which form ventilators of themselves. In each ward there is a constant supply of hot water, and the corridors are heated by means of pipes containing hot water. The water-closets, one on each floor, are external to the building, and approached through an ante-room, so as to be entirely cut off from the rest of the building. The soil is for many feet of the best gravel, and every attention has been paid to the drainage. As soon as a patient is able to be moved from the ward in which she has been delivered, she is placed in the convalescent ward, where she remains until she leaves the Hospital. As soon as three patients have been delivered in a ward, it is not again used till it has been well cleaned, as well as the beds and bedding; by this plan each ward remains vacant for ten days or a fortnight before patients are again received into it. When a case of puerperal fever occurs, the ward is freshly whitewashed, and the walls thoroughly washed down (in some cases repainted), the bedding purified and remade, and the ward not occupied again for at least a month. Everything has been done, both in the construction of the building and in the arrangements for the management of the patients, with a view to render them as little liable to disease as possible. In Section II., facts having more direct reference to the foetus were investigated. Of the total number of labours—7736—there were 2 cases of triplets and 84 twin births. Out of the 7650 single births, there were 235 abnormal presentations or 1 in every $32\frac{1}{2}$ cases, with a fatal result to the mother in 5 cases, or 1 in 47 cases; the head of the child, therefore, presented in 7415 instances: but of these the face presented in 15 cases, or 1 in $49\frac{1}{3}$ cases, and in 103 instances the child's head passed with its face looking towards the pubis of the mother. The funis was prolapsed in 22 cases, being met with once in every 348 cases; of the 22 cases 14 of the children were stillborn; in 12 cases it was prolapsed before the head, and of the children 9 were stillborn; in 6 before the feet, and of the children 3 were stillborn: 2 were cross births, and both stillborn. The upper extremity presented in 21 cases, or 1 in 364 cases, the breech in 139 instances, being 1 in 55 labours; of the children in these 139 cases, 88, or 1 in $3\frac{7}{10}$, were still born. The lower extremity presented 49 times, or one in 156 labours; 18 of the children, being 1 in $2\frac{1}{5}$, were stillborn. Placental presentations occurred 4 times, or 1 in $1912\frac{1}{2}$.

cases. Twin labours were then gone into, and a table given to show the presentation in each case; in by far the greater number of instances—namely, in 39 out of the 84 cases—both children presented naturally; next to this, cases in which the head of the first child, and the nates or inferior extremities of the second child presented, were most frequently met with—namely, in 22 cases. The first child presented with the nates in but 11 cases, and the lower extremity was similarly met with in but 6 cases. This section concluded with a notice of the operations. Craniotomy was employed in 21 cases, being 1 in $361\frac{1}{2}$ cases, and was followed by the death of the mother in six instances. Forceps were applied in 49 cases or 1 in 158 instances, and their application was followed by the death of the mother in 10 cases, or 1 in $15\frac{1}{2}$ cases.

(B) CONCERNING DISEASES OF WOMEN.

AET. 123.—*On the Employment of Apiol in Amenorrhœa and Dysmenorrhœa.*

By Dr. CORLIEU.

(Gaz. des Hôp.; and Edin. Med. Journ., August, 1864.)

Dr. Marrote, of the Hospital La Pitié, lately published a work on the utility of apiol (the active principle of parsley) in the treatment of these affections. Dr. Corlieu proposes to give an abstract of this publication, together with some observations suggested by his own practice. He says:—

“I have now employed apiol for eight years, sometimes successfully, sometimes not. I shall endeavour to point out the cases where it may be expected to prove useful.

“A. In all cases where the menstrual disorder depends upon the derangement of a vital element, where there is plethora or anaemia, apiol should not be used, for, being a nervous tonic, it will only aggravate the condition of the patient. But if the condition of chlorosis be removed, apiol may be prescribed with a good prospect of success. The following case will illustrate this:—A lady, thirty-eight years of age, of a lymphatic and nervous temperament, had suffered for three months from amenorrhœa, complicated with extreme chlorosis. Dr. Galligo at first ordered apiol, but without success. At a later period he combined it with chalybeates, which had previously done no good. The combined use of iron and apiol effected a cure. Dr. Marrote relates the following case:—Miss C—, eighteen years old, was of a lymphatic temperament. In childhood she had had measles, whooping-cough, and modified small-pox. Her skin was of a dead white, her face somewhat swelled; the gums were swelled and discoloured; she had very little appetite, and often vomited her food. She menstruated first when fourteen years old; for several months the discharge was white, afterwards it became of a reddish colour, but was accompanied by such severe uterine pains, that she was obliged to keep her bed. As she was to menstruate on the 18th of October, I

ordered her two capsules of apiol on the 15th, two on the 16th, and two on the 17th. On the 18th the menses appeared, though still in small quantity, but unaccompanied by colics or uterine pains; they only lasted two days, and the blood was still very pale. On the 21st, I ordered a chalybeate, which was continued till the 18th of November, when she resumed the apiol for three days. The menstruation was unaccompanied by pains; it continued three days, and the discharge was more coloured and more abundant.

"B. When the menstrual disorder depends upon a diathetic condition (dartres, scrofula, &c.) we must, by means of a specific treatment, such as bitters, cod-liver oil, preparations of iodine, sulphur, or arsenic, attack the principal malady. Apiol is of no use at first in these cases; but when the cure of the morbid diathesis has been effected, it may be employed with advantage in stimulating the torpid menstrual function.

"C. But it is chiefly in disorders which are under the influence of the nervous system that apiol is a heroic remedy, leaving far behind it all the emmenagogues hitherto employed. As a neurotonic it supplies to the nervous system the energy it has lost. Change of life, of habits, or of climate, often determine amenorrhœa. This is a fact which must not be forgotten, and which is well known to the physicians of boarding-schools and religious houses. This menstrual suppression is transitory; it lasts some months, and sometimes only gives rise to slight nervous disorders, or a slight oddity of character. In these cases, two, four, or at most six capsules of apiol will restore the menstrual flux. The following case was observed by Dr. Marrote:—

"Miss L. J—, seventeen years of age, born in London, arrived for the first time in Paris in October, 1861. She was a tall, handsome girl, of a good constitution and plethoric temperament; she had never had any serious illness. She menstruated for the first time at thirteen, but was very irregular up to fifteen. Although she never suffered from colics, or lumbar or inguinal pains, her periods were often delayed for a fortnight or three weeks; the blood, containing fibrin, was pretty abundant. From fifteen to seventeen years menstruation was quite regular. She then came to Paris, to complete her education. From the date of her arrival until the month of March of the following year, her menstruation was completely absent. Nevertheless no change had been observed in her general health, though I was informed that at times she became duller, more melancholy, and more irritable than usual, and that this state continued for a short time. Taking this as an indication, I administered apiol in the dose of one capsule night and morning in a spoonful of water, beginning on the 11th of March, which seemed to be indicated as a menstrual period. She thus took six capsules in three days. On the 14th of March the menses appeared without pain, and lasted four days. The lady of the establishment, accustomed to see apiol almost always succeed under these circumstances, did not repeat it the following month. On the 14th of April the menses returned, and lasted four days. On the 17th of May, the 16th of June, and the 21st of July, the same occurred. Miss L. J— returned to her family in London in perfect health and quite regular.

"The sudden application of cold during a menstrual period may suppress the discharge abruptly, and give rise to amenorrhœa, which may last for an indefinite time. In the month of January, 1861, I saw a young lady, seventeen years of age, who had menstruated for two years, but in whom, in consequence of a chill during a menstruation, the flow was suppressed. The belly became considerably enlarged : there was, in fact, an ascites, which could only be explained by the amenorrhœa. There was no albumen in the urine. I employed, without success, purgatives, sudorifics, chalybeates, and the ordinary emmenagogues. There was considerable pain at what should have been the menstrual periods. This state continued until the end of April. In May, capsules of apiol given night and morning restored the discharge, though at first it was pale and serous. Iron was continued, and from that time the abdomen diminished in size. The cure was complete.

"It would be easy to bring forward more cases, but the above may suffice. The point I wish to establish is, that apiol is the best emmenagogue with which we are acquainted in all cases where amenorrhœa or dysmenorrhœa have their origin in a disturbance of the nervous element. The principal condition for success in the use of apiol is in the choice of the proper moment for its administration. In almost all cases of amenorrhœa or dysmenorrhœa which depend upon an organic cause, the use of apiol is contra-indicated. This is not the place to lay down the differential diagnosis of these conditions. If apiol has succeeded in some cases of plethora, it has been because the plethora was not very considerable. 'In order,' says Dr. Marrote, 'that apiol may succeed, it is an essential condition that the pain which accompanies menstruation depend upon dysmenorrhœa properly so called, that is, on the vasmotor innervation of the womb. It has never succeeded in calming nervous pains, dull or acute, which were seated in branches of the lumbosacral nerves and especially in the uterus, pains which appear or become exaggerated at the menstrual period, and may at first sight simulate dysmenorrhœa proper.' Another condition for success in the use of apiol consists in choosing a time for its administration corresponding to a menstrual period. If the woman has not properly calculated the period, we may be enabled to discover it by noticing the sympathetic derangements which occur under these circumstances."

ART. 124.—*On the Treatment of Flexions and Versions of the Unimpregnated Uterus.*

By Dr. W. O. PRIESTLEY, Professor of Obstetric Medicine in King's College, London.

(*Medical Times and Gazette*, May 7 and 14, 1864.)

The following practical remarks are from an excellent clinical lecture in which the whole subject of flexions and versions of the unimpregnated uterus is fully gone into. After speaking of general treatment, Dr. Priestley proceeds :—

"Unfortunately, any method of treatment with which we are yet acquainted, and which promises any degree of success, entails penalties and inconveniences on the patient, which we are scarcely justified in subjecting her to, on the mere chance of prospective evils which may never arise. Some of the plans proposed oblige her to relinquish for the time all active occupations and duties, and may thus possibly deteriorate the general health. The use of mechanical expedients necessarily entails frequent vaginal examinations; and with some a risk is incurred of stirring up those very morbid processes which we are anxious to avoid. While, therefore, it is better not to interfere locally where displacement produces no special inconvenience—and this applies more particularly when the patients are unmarried women—the practitioner cannot be absolved from making no endeavour to relieve those patients who are less fortunate, and in whom uterine deviation without obvious uterine disease entails evil consequences. Wherever flexion or version predisposes to repeated attacks of uterine and ovarian congestion, or inflammation, or gives rise to serious discomfort, and sympathetic constitutional disturbance, whenever in married women, it seems unhappily to be the cause of sterility or abortion, he is, I think, bound to attempt something by way of palliation and cure.

"Proceeding on the principle that the least possible local interference which will answer the purpose required is best, we may, in a suitable case of retroversion or retroflexion, begin by restoring the womb to its natural position either by the finger pushed up behind the cervix in the vagina, or introduced into the rectum, or, where the finger is not sufficient, by the sound passed into the uterine cavity. The patient should at the same time be directed to keep the prone posture, or to lie on the side, with the pelvis inclined forwards. In this way we get the force of gravity to aid in keeping the uterus in its replaced position; and if in the movements of the patient it again relapses, we may lift it up from time to time, and gradually accustom the organ to keep *in situ*. Throughout, great attention should be given to improve the general health, to regulate the condition of the bowels, and to remove any undue congestion of the pelvic organs which may arise from an inactive state of the liver. We may employ likewise the cold hip bath, the cold douche into the vagina, or some astringent injection, with the object of giving tonicity to the parts and improving the relaxed condition of the uterine ligaments. The observance of these simple rules will occasionally be quite sufficient to remedy the displacement, and restore the patient to health and comfort, particularly if the displacement be recent or be the result of accident. It must, however, be confessed that the instances in which cure is effected by these means are not numerous. In the majority of cases, the fundus uteri falls back to its abnormal position as soon as the force which raised it is withdrawn, and a more prolonged and constant use of the prone couch, as recommended by some authorities, while it does not remedy the malposition, injuriously affects the general health. Under these circumstances one naturally turns to the idea of some mechanical contrivance which imitates the finger or sound used in replacing the womb, but which, instead of being a temporary force,

may be a permanent support, and at the same time permit, if possible, the patient to move about. With this object various instruments have been invented by Drs. Simpson, Valleix, Detschy, Graily Hewitt, and Routh. Those known as Dr. Simpson's, and described by him in an able memoir in the year 1848, have received the largest share of attention. Dr. Simpson's pessaries consist essentially of a stem to be passed into the uterine cavity, and retained there, either by a base sufficiently large to be grasped by the vagina, or by the union of a wire framework which lies externally on the symphysis pubis, with the base of the stem worn in the interior of the womb. I have seen pessaries constructed on this principle extensively employed both by Dr. Simpson himself and by the late M. Valleix in Paris, and in many cases I have seen notable benefit result. Much opposition has, however, been raised both in this country and on the continent to their general adoption; and in a discussion before the French Academy of Medicine ten years ago, the most disastrous consequences were stated occasionally to have occurred. It has not fallen to my lot to witness any results so serious as those mentioned during the discussion alluded to, yet I have seen sometimes much irritation, pain, and haemorrhage follow the introduction of an intra-uterine pessary, and necessitate its withdrawal. The interior of the uterus in some women is so sensitive that it may be compared with the conjunctiva of the eye,—the slightest touch giving pain; the introduction of the uterine sound being followed by severe suffering, and the retention of an intra-uterine pessary being altogether intolerable. Besides this, the introduction of these supports is not a very easy matter, and their adjustment requires both skill and experience. While, therefore, I should not hesitate to have recourse to an intra-uterine pessary in a suitable case, where other means had been unsuccessfully employed, I prefer commonly to use some support which does not require to enter the uterine cavity, and is therefore less likely to be followed by unpleasant consequences, and to call for a suspension of treatment.

"I have found best to answer this purpose a pessary made of gutta-percha, which may have a wire framework, thus rendering it amenable to any alteration of curve which the case requires. The form (Fig. 1) is that of a loop, ending below in a stem, which projects slightly beyond the vulva, and to this stem elastic straps are attached, which are fastened behind and before to a waistband. The figure is somewhat like the letter S from which the upper limb has been cut off, the lower limb sweeping over the edge of the perinæum. The upper extremity is passed into the vagina and pushed up behind the cervix uteri (Fig. 2), thus raising the fundus and keeping it upright, the elastic thigh-straps retaining it in position without exerting undue pressure. Ordinarily, it is desirable to raise the uterus, and then introduce the support to keep it in position; but this is not always possible, and is not essentially necessary, as the pressure of the pessary will gradually lift up the fundus. My friend Dr. G. de Mussy has suggested that if the upper end were covered by an air cushion, it would not only give more equable support to the uterus, but by filling the space occupied by the reflexed fundus, more

effectually counteract its backward tendency. I have, consequently, had a small india-rubber pouch attached to the top, which can be inflated by an elastic bottle after introduction, and thus fulfil the indications mentioned (Fig 3). If rectocele exists as well

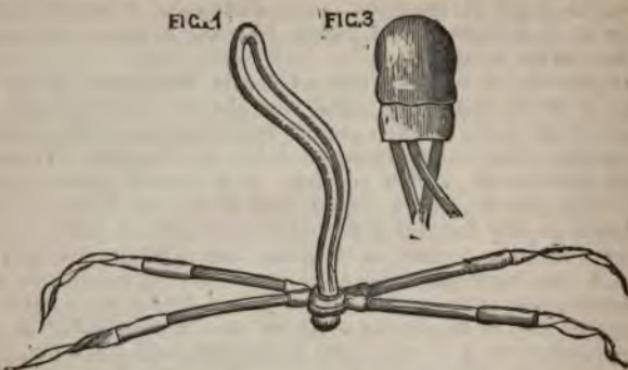


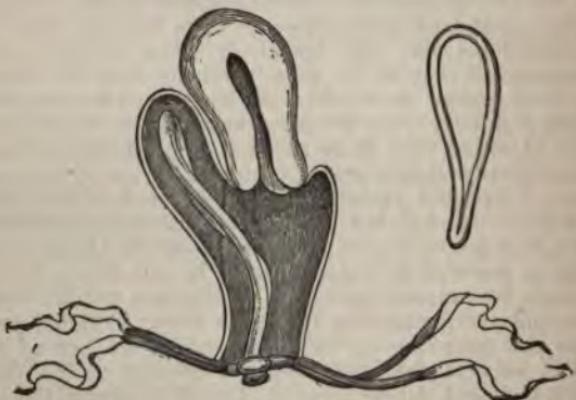
FIG. 1.

FIG. 3.

as retroflexion or retroversion, the loop of the pessary may be entirely covered over with india-rubber, and it thus answers the double purpose of supporting the uterus and the rectocele. In

FIG. 2.

FIG. 4.



ordinary cases of retroflexion it is best to dispense with caoutchouc additions, as they are not necessary, and, besides being undurable, provoke more profuse discharges, and produce a greater distension of the vagina. The advantages of this form of pessary are, that it generally produces little discomfort, it distends and relaxes the vagina in the least degree possible, and it permits the patient to take exercise. In most cases, it not only efficiently supports the

uterus, but it counteracts also the tendency to prolapse of the ovary, which has been mentioned as one of the troublesome complications and causes of suffering in retroflexion and retroversion. It is probably in those cases where one or both ovaries are prolapsed and tender, or where the uterus, notwithstanding suitable treatment, remains large and yet sensitive, that the addition of the india-rubber cushion is most desirable. In other instances, as before mentioned, it is best to dispense with it.

"Another excellent form of pessary is that invented by Dr. Hodge of Philadelphia, and called by him the lever-pessary. It is made of vulcanite or gutta-percha, and is somewhat like a horse-shoe in shape, with a curve somewhat like the letter S. There are two principal forms of this pessary. It is passed up behind the cervix uteri and is altogether contained in the vagina, doing away with the necessity for straps and bandages. This latter is an important advantage, which is, however, counterbalanced, in some degree, by the instrument not reaching quite high enough to keep the uterus erect, and by the tendency it has in some patients to slip out of the vagina during defæcation. All forms of vaginal pessary, even if they do not fulfil entirely the purpose of keeping the uterus in its normal position, may act beneficially by steadyng the organ and warding off the effects of concussion. In this way the ordinary round, inflated, ball pessary, introduced into the vagina, by raising and supporting the entire womb, often gives marked relief in any of the varieties of uterine deviation. Pessaries of the kind I have described may be worn for two, three, or more months, care being taken to remove them occasionally for the sake of cleanliness; and it is generally desirable to withdraw them during the menstrual periods. My friend Dr. Moir of Edinburgh has recently proposed that, in the more obstinate cases of retroflexion without enlargement, we should dilate the uterus with a graduated series of sponge-tents, thus bringing the womb artificially into the condition observed after early miscarriage, and then allow it to contract gradually upon a properly-adapted series of intra-uterine pessaries. I have not yet attempted this procedure, but Dr. Moir speaks well of it, and he is a careful and trustworthy observer.

"When the uterus is anteflexed or anteverted, the same kind of intra-uterine pessaries as those employed in retroflexion and retroversion have been recommended by Dr. Simpson and M. Valleix as equally applicable for keeping the womb erect. Vaginal pessaries which do not enter the cavity of the womb are not so readily adapted to support the fundus when it inclines to fall forwards as when it inclines to fall backwards. The attachment of the bladder to the anterior wall directly interferes with the efficient adjustment of the support I have spoken of as useful in cases of retroflexion and retroversion; and the pressure on the bladder during its distension would be a source of serious inconvenience. If, therefore, we relinquish intra-uterine pessaries, we have to content ourselves either with introducing a simple round, inflated ball into the vagina for the purpose of raising and giving a general support to the uterus, or we may employ some of the varieties of pessary which have been

devised with the object of keeping the cervix uteri forward towards the symphysis pubis, and in this way using the lower segment of the womb as a sort of lever to elevate the fundus. A disc or ring of ivory or wood, so adjusted across the pelvis that the cervix is retained in its centre, was used by the late Dr. Dewees and others.

"I have employed advantageously with the same view a pear-shaped gutta-percha loop (Fig. 4), first showed to me by Dr. Simpson, and which passes up behind the cervix, supporting it and drawing it forward, the narrowed and lower extremity which lies over the edge of the perinæum being free and unattached to bandages or thigh straps.

"The pessaries of Dr. Hodge would no doubt fulfil very well the same indication. The simple loop (Fig. 4) I have also used advantageously in some of the less obstinate cases of retroflexion.

"In conclusion, I may add that, in all the forms of uterine displacement, great care should be taken that treatment interferes the least possible with the general health; and that in all cases where there are evidences of feebleness, anaemia, and mental depression, these should be counteracted by appropriate means."

**ART. 125.—Successful Case of Double Ovariotomy in which
135 Injections were made into the Peritoneal Cavity
during 78 Days.**

By Dr. E. R. PEASLEE, New York.

(*Amer. Quart. Journ. of Med. Science*, July, 1864.)

The subject of the following operation was Mrs. E. H.—, of Thetford, Vt., thirty-nine years of age, the mother of two children. The tumour was one of the polycystic variety, and it and the operation present nothing especially worthy of notice. The history of the case after the operation, and the remarks following, will sufficiently show what were the points of interest. The operation was performed on the 3rd September, 1863.

The whole operation lasted very nearly five hours; during the whole of which time the patient remained insensible under the ether. The pulse was now 120, and not very weak. She had no appearance of collapse, soon recovered from the effects of the ether, and had no subsequent nausea or retching.

McMunn's elix. opii gtt. xxv. were given just after she was put in bed, and the following is a condensed report of her progress after the operation. My object is more especially to call attention to the after-treatment. I will here state, however, that though I had of course expected to use the catheter for the first few days, the patient insisted on evacuating the bladder by her own efforts at 9.30 o'clock (then $\frac{5}{3}$ ij.), and did so throughout the progress of the case, averaging about $\frac{5}{3}$ ij. to $\frac{5}{3}$ ij. every three hours, or $\frac{5}{3}$ xvj. to $\frac{5}{3}$ xxiv. per diem.

10.30 P.M.—Pulse 112 and regular. Skin natural. Reaction well established. Elix. opii gtt. xx. Took a little milk-porridge. 12 o'clock. Has slept a very little, but is quite restless. Elix. opii gtt. xxx. 3 A.M. Pulse

168. More quiet now and has but little pain. Compress changed. 6 A.M. Pulse 120 and fuller. 7.30 A.M. Elix. opii gtt. xxx. "Feels as though she was going to get well."

Sept. 4th. Morning after the operation, 9 o'clock.—Pulse 120 and of fair strength. Countenance pale, but spirits good. No nausea. 11 o'clock. Elix. opii, gtt. xx. 2 P.M. Pulse the same. Has slept half of the last three hours quietly. Skin good. Is somewhat thirsty, and feels sick a little. Turned her on her left side. Takes two tablespoonfuls of milk-porridge every two hours. 6 P.M. Elix. opii gtt. xx; just after taking it vomited very easily. 7.30 P.M. Pulse 125, steady, but not as strong as in the morning. Still a little thirsty. Elix. opii gtt. xxv. 9.30 P.M. Vomited again at 9 A.M., but feels better for it. Pulse a little weaker. Elix. opii gtt. xxv. 12 noon. Has been sleeping a little. Pulse 126 and weak; respiration 32. Brandy 3j; beef-tea three dessertspoonfuls—repeated soon. Expression anxious. 1 A.M. No more nausea. Doing better. Elix. opii gtt. xxv. Brandy, 3j. 3 A.M. Has slept about half of the last two hours. Pulse 124 and very weak. "Feels very sleepy and tired," but is constantly moving her limbs about because of their aching. Brandy 3j, and beef-tea. 4 A.M. Injection of brandy 3iv, and beef-tea 3ij. 6.30 A.M. Pulse 120, with 12 intermissions per minute—still weak but better; respiration 24. Not much pain, but feels very tired. Has slept quite well. Elix. opii gtt. xx.

Second day after the operation, 9 A.M.—Pulse 118 and stronger; skin very good; pupils unaffected by opium; abdomen tympanitic, but no tenderness. 12 noon. Pulse the same—no intermissions; skin moist and of good temperature; expression of countenance better than last evening. Elix. opii gtt. xx. Brandy 3j every hour since early morning and beef-tea frequently. 6 P.M. Pulse 120. Clothing changed causing very little fatigue. Took pil. opii j at 3 P.M. Beef-tea and brandy not quite as often—the stomach being somewhat irritable. 8 P.M. Injection of beef-tea, 3ij, brandy 3ij, and elix. opii gtt. xxx. 3 A.M. Pulse the same. Has had elix. opii gtt. xx and brandy 3j every two hours. Injection of beef-tea 3iiss, brandy 3j, elix. opii gtt. xx.

Third day, 9 A.M.—Pulse 118 and better. Is comfortable; no increase of tympanitis, no tenderness; tongue moist, and thirst less. Has slept some. 9 P.M. Pulse 118. No nausea. Abdomen very large and tympanitic, but not tense. Troubled with eructation, but otherwise comfortable.

Fourth day, 9 A.M.—Stomach irritable; has vomited some, and food and medicine have been given per rectum. Tympanites has been very troublesome, and a tube was introduced into the rectum, affording relief. Soda bicarb. occasionally for acid stomach. She asked for cracker and tea, and was allowed to have it. Pulse 120; tongue good. Gas passed per rectum. 3 P.M. Quite restless. Tongue a little dry. Has not taken as much nourishment to-day as yesterday. Injection beef-tea 3iv, brandy 3ij, and elix. opii gtt. xxx. To have elix. opii, gtt. xv every two hours. *Some blood-stained serum escaped by the side of the ligature.* 9 P.M. Very comfortable. Pulse 120 and good; tongue better. Clothes a good deal stained by the blood-coloured serous fluid from the peritoneal cavity. Gas passed several times per rectum.

Fifth day, 9 A.M.—Pulse the same. Comfortable night. Takes cracker moistened with brandy-and-water, and a little beef-tea, and has the nutritive injections every four hours. Less tympanitis. Weak, but does not complain of feeling tired. The blood-stained serum oozes out freely. Urinary secretion abundant. 9 P.M. Oozing continues and its colour is a little darker. Talks in her sleep.

Sixth day, 9 A.M.—Pulse 120 and good; has been sleeping and feels very comfortable. Talked in her sleep. The fluid now oozes through an opening above the umbilicus, which has not closed up. Its colour deepens. Doubtless there are clots in the peritoneal cavity. Tympanitis no longer troublesome.

Seventh day, 9 A.M.—Pulse 120. Feels weak and looks pale; tongue a little dry in the middle. The dark serum, now quite fetid, continues to ooze freely. Only 3ss of brandy during the night. Talks in her sleep; has vertigo, but insists that all is right. 2 P.M. Has been restless and feels tired; countenance anxious; tongue rather dry. Head feels confused. Two good alvine evacuations produced by an enema of soap and water. The signs of blood-poisoning being now sufficiently evident, I decide to inject the peritoneal cavity at the next visit. 7.30 P.M. Injected the abdomen with a pint of blood-warm water and obtained over half a pint of pretty thick, bloody matter; and some also flowed out after the tube was withdrawn. 10 P.M. Pulse 112 and weak. A little wandering when partly asleep. Has been having the injections of beef-tea and brandy, and the elix. opii regularly.

Eighth day, 9 A.M.—Had a rather bad night. Pulse has been somewhat irregular and quite weak; skin more dry; tongue dry in the centre, and countenance dusky. Is evidently sinking. Had quinine gr. j. at 4 P.M. and has had brandy 5j every hour, in addition to 3v by injection. Injected the peritoneal cavity as before, and obtained 3iv black and not very fetid blood, and then freely washed out the cavity. The pulse had risen from 112 at 2 A.M to 130 at 2 P.M.; weaker and more irregular. 2 P.M. Injected four pints of warm water (two at once) into the peritoneal cavity, and it brought away a great deal of highly-stained fluid of a dark colour; no indications of recent haemorrhage. Removed two of the eight silver sutures, and one double silk suture below the umbilicus. Patient fatigued by the prolonged operation of one hour. Pulse now 140; restless. Beef-tea and quinine by injection with elix. opii gtt. xv. Brandy 3j every hour. 5 P.M. Elix. opii gtt. xxv. 9 P.M. Pulse 135, feeble. Has been more quiet. A little oozing of a very dark, almost black, fluid. Injected one quart of water with liq. sodæ chlorinatæ 3ij, and it returned, towards the last, of a lighter colour than that which oozed out. Tympanitis returning. 10.30 P.M. Elix. opii gtt. xxx. 12 P.M. Injection of brandy 3ij, quinine grs. ij, with beef-tea every four hours. Pulse 136 and very weak. 4 A.M. Injection as before, in addition to the 3j of brandy every hour by the mouth. Pulse irregular, intermitting; tongue has a dark centre. Oozing still continues. 7 A.M. Pulse 134 and more irregular. Delirious. Sent her attendant out of the room, and arose and sat up on the edge of the bed.

Ninth day, 9 A.M.—Pulse 130, feeble; countenance not quite as bad; tongue brown, but not quite as dry. Injected her abdomen, and removed half a pint of very dark, fetid fluid. Leave half a pint of water, containing 3j of the chloride of soda, in the cavity. 2 P.M. Injected thoroughly, and brought out much dark, flaky matter. Left 3iss of the soda in. Pulse 127, and fuller; tongue better, and more moist. Clothing changed. 9 P.M. Pulse 115, and better. Quiet and comfortable since 2 P.M. Four spontaneous alvine evacuations during the day. Suppository of opium gr. j. Oozing still of dark, offensive fluid.

Tenth day, 9 A.M.—Slept two-thirds of the night. Pulse 110, and of very fair force; two alvine evacuations since last report, bringing away much flatus and affording great relief by removing distension. Injections brought out more of the dark matter, but not as much as before, and not so offensive. 4 P.M. Pulse 104; tongue better. A little oozing still of thick, very dark fluid. Injected two quarts of water in all, and it came

away less turbid than before. 10 P.M. Injected again, using the chloride of soda freely. Another pin, two silver sutures, and the lowest double silk suture removed. Had during the day elix. opii gtt. xxv, and brandy 3j every hour, with quinine gr. j every four hours as usual. Chewed some beef at noon.

Eleventh day, 9 A.M. Injected as usual; fluid comes out clearer. 3 P.M. Has been growing more and more restless since morning; tongue drier; pulse 120. Washed out the peritoneal cavity thoroughly, obtaining a darker fluid. Pulse, after the operation, 112. To take quinine gr. j every three hours, beef-tea by the stomach freely, and per rectum, with elix. opii gtt. xxv. Enema repeated at 6 P.M. 10 P.M. Lighter coloured fluid obtained by the injection. Two more silver sutures removed.

Twelfth day, 9 A.M.—Pulse rather weak during the night. Has been taking brandy 3j every hour. Better now. Fluid removed, clearer, and nearly odourless. Appetite good. 2 P.M. Tongue cleaner at the edges, but still dark and heavily coated in the centre. About 3ss of thick, somewhat offensive pus obtained from the abdominal cavity, some of it flowing out through the bougie, and some being drawn out by the syringe. None of the red, flaky matter obtained. 3ss chloride soda left in. 10 P.M. Fluid quite clear. Pulse 102 before injection, and 96 after it. No opium since 3 P.M., then gtt. xx. Better than yesterday. One needle removed. Takes brandy 3j every hour.

Thirteenth day, 9 A.M.—Good night till 4 A.M.; then two alvine discharges; the first natural, the second fluid. Enema of beef-tea, with tinct. opii gtt. xl. Very comfortable now. Mutton chop for breakfast; appetite good. 4 P.M. 3ss of pus escaped around the tube on pressure, and the injection brought away some more. Last needle removed. 10 P.M. Pulse 98. Very comfortable day. Very little opium during the last twenty-four hours. Takes brandy 3j every hour and a half. A little purulent fluid obtained by injection.

Fifteenth day, 9 P.M.—3j good pus obtained by pressure. Washed out the cavity thoroughly, the fluid at first containing considerable fetid pus, but being quite clear at last. No tenderness of the abdomen at any time. Injected but twice to-day. Tinct. cinch. comp. 3j three times a day instead of the quinine. To take brandy 3j every two hours.

Sixteenth day, 9 P.M.—Pulse 96, good; tongue quite sore from sudden clearing off of the coat. Tinct. cinch. comp. being rejected by the stomach, to take quinine gr. j every four hours again. 3ss pus pressed out at noon, and only 3j to-night, but it was dark and had some fetor. Injected but twice to-day, and so on to the twentieth day.

Seventeenth day, 9 A.M.—Pulse 106, and weaker; countenance more pale; some sorde on the teeth; little appetite. On the whole failing a little. Complains of pain on passing bougie down towards the ligatures, also on filling the cavity with water. Pus more fetid. Removed more of the dark, thick matter, like that removed at first. To take liq. sodæ chlorinatæ gtt. vj every three hours. Had brandy 3j every hour during the night. 9 P.M. Pulse 96, and better. About 3j of pus obtained by injecting and allowing the fluid to flow out; no fetor. She feels comfortable and has no pain. Bowels have been rather loose during the last three days, there having been two or three thin discharges daily; one good one to-day.

Eighteenth day, 9 A.M.—Pulse 96, rather weak; tongue less red, but clean; not much appetite. Injected water came away thicker and darker than usual. 4 P.M. Pulse 100, and quite weak; some nausea most of the time. 3ss by pressure. Fluid came away in the morning, and with it one little slough (from the omentum?). 10 P.M. Has been more comfortable since last injection. Sat bolstered up in bed half an hour. No sorde

on teeth now ; no nausea. To take now tinct. opii gtt. xv in the enema of beef-tea.

Nineteenth day, 9 P.M.—Very comfortable to-day. Less pus, and that odourless.

Sept. 23rd. Twentieth day, 9 P.M.—Pulse 88. Best day yet. Have injected three times, but have obtained only a little pus. She ate a little beef-steak with good relish. Discontinue the chloride of soda.

Twenty-first day, 9 A.M.—Slept considerably last night, and has been better to-day. Beef-steak and rice for dinner. Pulse 92. Was removed to the sofa, and remained there three hours. No opium during the last twenty-four hours. Fluid clearer. To take brandy 5j, with quinine gr. j, every fourth hour.

Twenty-third day, 10 P.M.—Pulse 88 and good. Improving a little. She took this morning hydrarg. cum creta grs. iij, with pulv. rhei grs. iv, and two good evacuations resulted. But little pus oozes out now, and but little is obtained by injecting, and that nearly odourless.

Sept. 30th. Twenty-seventh day, 9 P.M.—She had a slight relapse during the last two days, and there was more of the pus obtained by injecting, and the odour was offensive. The stimulants were given in increased doses, and the chloride of soda injected. Better to-day every way. Sat up in a rocking-chair fifteen minutes without fatigue. Discontinue the chloride of soda by stomach. Tinct. cinch. comp. 5j instead of quinine, which the stomach seems to reject. Has been injected twice daily since the twentieth day.

Oct. 1st. Twenty-eighth day.—At this time I returned to New York, leaving Mrs. H—— in the care of my assistant, Dr. L. B. How, now of Manchester, N. H., and to him I am indebted for the following account of her progress :

“*Twenty-ninth day, 9 P.M.*—Pulse 88, and of very fair strength. Rather more pus to-day than usual by injection, but there was very little odour to it. Sat up half an hour to eat a breakfast of beef-steak, baked potato, and baked apple. Sat up another half hour in the afternoon. Sleeps better than at any other time during the last two years. The tincture of cinchona not seeming to agree with her stomach, the citrate of quinine and iron was substituted for it.

“From this time she improved steadily, but slowly ; her convalescence being considerably retarded by numerous exceedingly troublesome boils in the gluteal region and on the abdomen. The injections were employed twice daily (till the forty-seventh day) bringing away about 5ij of pus each time. As the opening through the abdominal wall seemed to be closing up, it was necessary to leave the bougie in for a short time after each operation, and soon, as the amount of pus decreased, a smaller bougie was substituted. All the pus now came, apparently, from the inner edges of the incision below the hernia, where, after the operation, it was impossible to perfectly coaptate the lips of the wound. It was all deposited outside of the omentum, *i.e.*, between it and the abdominal wall, and midway between the upper and lower openings. Very little, if any of it could have come from the pedicles. The bowels were kept regular by ripe fruit, and no cathartics were required.

“*Oct. 12th. Thirty-ninth day.* With some assistance she walked across the room to the sofa, and sat up half an hour.

“*Forty-third day.*—She arose and dressed herself without assistance, and sat up two and a half hours. One ligature removed from the pedicle of the right side. The abdominal walls have contracted so much that only 5ij of water can be ejected at once without occasioning discomfort. Very little pus is secreted around the upper opening, and but little around the ligatures

in the lower one. About 3ij of pus of normal appearance are drawn out through the tube with the syringe daily. A little of the chloride soda was used each time."

On the 20th of October, forty-seven days after the operation, she returned to her home in Thetford, Vt., a distance of fifteen miles, where she came again under the care of Dr. Worcester, to whom I am indebted for a report of her subsequent progress.

Owing partly to fatigue from the journey, and partly to a slight attack of diarrhoea, she was unable to sit up till October 27.

Nov. 3rd.—She sat up half a day, and walked about the house a little. *The second ligature* of the right pedicle came away. On introducing the tube this morning about 3ij of pus escaped, and a like amount at night. Water injected passes out at the lower opening at once, none of it being retained.

From this time the amount of pus gradually diminished till November 26, when, none having been secreted in the abdominal cavity for forty-eight hours, the upper opening was allowed to close up, and it did so rapidly.

Dec. 3rd.—The *ninety-first day* after the operation, *a third ligature was removed*, and three days later the *fourth and last*, after which the lower opening closed very speedily. The injections were used by Dr. Worcester twice daily during the first few days, and subsequently once. The patient, doubtless, lengthened the period of her convalescence by taking improper food, contrary to directions. She is now, March 1, complaining only of a slight lameness, which she considers rheumatic.

" *Remarks.*—1. This is my third case of double ovariotomy, all successful. In the report of my preceding case, I expressed the belief that my *first* was the first reported in this country. I have since learned that the priority belongs to Dr. J. L. Atlee, of Lancaster, Pa.

" 2. Decided tympanitis has occurred after three of my operations; commencing, as in this case, on the second day, and giving no trouble after the fourth or fifth. It was not accompanied by tenderness of the abdomen in either case; and rapidly abated as soon as the gas began to pass *per rectum*.

" 3. Without my previous experience, I should have decided not to remove the tumour in this case, on perceiving the extreme vascularity of the omentum, and the extent and firmness of the adhesions. For though I did not doubt I might arrest the haemorrhage to such an extent, before closing the incision, that the patient would not die of subsequent loss of blood, I felt positive that some oozing of blood into the peritoneal cavity must subsequently ensue, and thus blood poisoning be produced. The results, however, of injections into the peritoneal cavity, in two cases previously reported, assured me that even if septicæmia should occur, I need not consider the patient as beyond my control, and therefore I decided to proceed.

" 4. No positive symptoms of internal haemorrhage presented themselves; though the sickness and great restlessness confirmed my previous expectations in this respect, and the use of the injections showed that nearly a pint of blood must have been slowly poured out after the incision was closed. The symptoms of septicæmia were first perceived on the fourth day, became decided on

the sixth, and were met by the use of injections on the seventh day. They were decidedly checked after the eighth day, and diminished till the fifteenth, when they again increased to the seventeenth; after which time they were easily controlled till the injections were discontinued. The effect of sulph. quinine in raising the patient above the septic influence (and also in neutralizing it, perhaps) was most striking. If the doses were either diminished or given at longer intervals, the symptoms were at once sure to return with increased intensity. The effect also of the liquor sodae chlorinate, given internally, in neutralizing the poison, was equally apparent. I feel positive in regard to these two points, and consider them the two great remedies for septicæmia.

"5. We can account for the return of the symptoms of septicæmia in an aggravated form, after having been so well controlled (after the ninth day) by the injections, I think, only by recognising two independent sources of blood-poisoning. (1.) The blood in the peritoneal cavity caused the symptoms which increased from the fourth to the ninth day. But this cause was well under control by the twelfth day, when the second appeared, viz. the formation of *pus*, which on this day first escaped around and through the bougie, and which necessitated the injections even to the eighty-fourth day after the operation. I expected much difficulty in removing the blood, since it was *behind* the omentum, and the latter was very *wide* as well as long and thick; but by always carrying the bougie obliquely towards the lumbar region, that difficulty was at last overcome. But whence came the *pus*? I doubt not from the granulating surface between the edges of the incision, internally; it having been impossible perfectly to coaptate them, on account of the thickness of the abdominal walls below the umbilicus, as before stated. Hence the suppuration continued till the union was completed internally. The *pus*, therefore, was formed in *front* of the omentum, though it often passed over and behind it, before it was removed.

"The whole number of times the peritoneal cavity was injected before the patient returned to Dr. Worcester (41 days) was 89. He repeated the operation 46 times, making 135 injections in 78 days. In the two other cases treated in this way, I continued the injections 7 days and 59 days. Without their use, I feel confident that all three of these patients would have succumbed.

"7. So large an amount of opiates as was administered in this case is, I think, very seldom justifiable. Enough should be given, in every case, to control pain and restlessness, and secure sleep, and *no more*. In some cases, therefore, I have not given more than a single daily dose during the first three or four days only; while in this case, from 145 drops of McMunn's elix. down to 90 drops were given every twenty-four hours during the first five days. I have always given the opiates, and the nourishment, *per rectum*, if the stomach is irritable."

AET. 126.—*On the Origin, Structure, and Mode of Development of the Multilocular Cysts of the Ovaries.*

By Dr. WILSON FOX, Professor of Pathological Anatomy at University College, London, and Assistant-Physician to University College Hospital.

(*Proceedings of the Royal Med.-Chirurg. Soc.*, June 20, 1864.)

The first division of the paper consists of a *résumé* of the views hitherto held with regard to the origin of the cysts. The author considers that the opinions hitherto expressed on this point may be divided into two chief classes.

1. Those which attribute the cysts of the ovary either to morbidly affected Graafian vesicles, or to secondary formations from these structures.

2. Those which ascribe the multilocular forms to a morbid process arising in the stroma of the ovary, independently of the Graafian vesicles.

Under the second category there is a great variety and discrepancy of opinions.

With regard to the former, it has long been doubted whether the number of the Graafian follicles normally existing in the ovary is sufficient to account for the whole of the cysts sometimes found in these tumours: while the proof of any fresh formation of Graafian vesicles taking place in the adult has hitherto been of a very dubious kind, nor has any account been furnished of the mode in which secondary cyst formations proceed from them.

The author has studied these conditions in fifteen of the so-called "colloid cysts" of the ovary, for the opportunities of examining most of which he has been indebted to the kindness of Mr. Spencer Wells. He believes that all primary cysts of the ovary originate in the destruction of the ovum and subsequent accumulations of fluid in the follicle, the membrana granulosa acting as a secreting structure. From these cysts secondary cysts may originate in various ways, all of which, however, may be referred to one common type.

Class A.—Cysts give off long tubular processes, lined by an epithelium similar to that of the cyst whence they spring; one cyst may give off two or three such processes at various parts of its circumference. These undergo constrictions in their course, and thus form secondary cysts. These processes and the cysts from which they spring are most easily found in the more dense parts of the stroma.

Class B.—Thin-walled cysts give off diverticula analogous to those by which the lungs, the thyroid, and some other glandular organs, both of the gusto-pulmonary and genito-urinary system, originate in the embryo. These diverticula, which open by a narrow neck into the cavity of the parent cyst, expand as large pouches on its external surface, protruding into other and adjacent cysts. The neck may either expand into a large opening, or may become constricted, in which latter case the original communication is

destroyed. One cyst may in this manner give off numerous diverticula.

These varieties (A and B) usually coexist in different parts of the same tumour. They were found by the author in three out of the fifteen tumours examined by him. They give rise to very compound structures, but not to the dense masses to which the names of "alveolar degeneration," "cystoid disease," and "adenoid tumours" of the ovary have been given. This variety (or Class C) results from the formation, on the inner surface of the cyst wall, of a series of tubular glands, analogous to those of the uterus, or the crypts of Lieberkühn, or the glands of the stomach. They are formed (1) by the epithelium lining the parent cyst becoming stratified, and in its superficial layers assuming a columnar character. (2) Into this stratified epithelium, papillæ formed of connective tissue spring from the stroma of the ovary, in each of which a loop of vessels is formed. A series of densely-clustered villi is thus produced, which are converted into tubular glands by the growth upwards around these bases of the stroma of the ovary. The glands may become compound at their bases by secondary villi arising in them. They may be converted into simple cysts by the closure of their orifices; but more commonly the upward growth of the stroma surpasses that of the villi in which their summits end, and the glands become completely shut off and enclosed in the stroma, forming groups of a very compound form, of tubular structure, lined by a secreting epithelium embedded in the wall of the parent cyst. When distended by further secretion they form the smaller and larger multilocular cysts scattered on the inner wall of the parent cyst. Other modes of cyst-formation resulting in dense cystoid masses were traced by the author to these structures.

Class D refers to the cyst found in the cauliflower papillary or dendritic growths which spring from the interior of parent cysts. These growths originate in a number of delicate papillæ growing from a common basis, and uniting to form larger masses. They consist of a delicate stroma, derived from that of the parent cyst-wall, a loop of vessels, and a covering of epithelium. The irregularity of their growth causes spaces to be enclosed by them, lined by a secreting epithelium, and which, when completely shut off, become cysts. Various illustrations were given of this process. The author considers that in no case are the secondary cysts in the cauliflower growths of the ovary derived from single epithelium cells.

The author then refers to the observations of Drs. Pflüger and Billroth on the origin of the Graafian follicles from tubular structures found in the embryonic condition of the ovary; and says that though not able fully to corroborate all Dr. Pflüger's views from his own observations, he has convinced himself that the Graafian follicles originate in tubular structures. He regards these cysts as resulting from a renewal in the adult of the early mode of development of the Graafian vesicle with various morbid aberrations from the type of embryonic growth; and thinks they must therefore be placed in the same category with other cystic tumours growing in structures

having tubular glands and ducts, especially with those of the mamma, testicle, and thyroid gland. He regards the cysts mentioned under Class D as presenting essentially the same type, inasmuch as the large papillary and cauliflower masses can only be regarded, similarly to the Haversian fringes of synovial membranes, as everted glandular structures. He has not had any opportunities of examining any multilocular cysts of the ovary containing dermoid structures; but, inasmuch as these have been shown to contain both normal hair follicles and sebaceous and sudoriparous glands,—all of which structures are the frequent seat of cyst formations,—he believes that they will be proved to follow the same laws of growth as the colloid cysts. The author, from chemical examinations of the fluid contents of the cysts, has been led to regard the so-called colloid matter found in them, as the result of alterations depending on the varying conditions of pressure under which they are secreted from the inner surface; and he believes that this matter cannot be considered as the result of any special form of degeneration of the tissue of the ovary.

The method which the author has pursued in studying the development of the cysts of the Classes A C D, has been to make sections in the recent state with a Valentini's knife through various parts of the stroma. The glands of Class C are best displayed by sections made vertically to the inner surface of the cyst-wall. Observations on Classes C and D are much facilitated by hardening the tissues in chromic acid solution of two per cent., and subsequently treating sections made by a sharp razor with liquor of soda and glycerine.

ART. 127.—On a New Mode of treating Vesico-Vaginal Fistula.

By Dr. ALFRED MEADOWS, Physician-Accoucheur
to the General Lying-in Hospital.

(*Proceedings of the Obstet. Soc. of London*, May 4, 1864; and
Dub. Med. Press, June, 1864.)

It was contended in this paper, that the usual practice of keeping the patient in bed for two or three weeks after the operation for the cure of vesico-vaginal fistula is unnecessary, and that, on the contrary, she may be allowed with perfect safety to go about as usual immediately after the operation. The author showed that the reason given for the former practice—viz., that the parts should be kept quiet—is as fully attended to in the plan suggested as in that usually followed, because the movements of the body do not interfere with the quietude of that particular portion of the floor of the bladder where the fistula existed, there being no muscles in this region which can by their attachments prejudicially affect the part in question. With regard to the second consideration—that the urine should be kept from the surface of the fistula, either by the constant employment of the catheter or by its frequent use—the author exposed the fallacy of this argument by briefly reviewing the

circumstances which exist after every operation of this kind. At first the bladder is quite empty, but, as urine gradually flows into it, the organ becomes slowly distended; and the very fact of this distension taking place by the uniform pressure of the urine, proves that contact of that fluid with every part of the bladder-wall cannot be avoided; no position of the patient can prevent it, and consequently the recumbent posture is not needed on this account, nor is the use of the catheter of any service. Two cases were detailed in which the plan here suggested by the author had been carried out with perfect success. In one, where chloroform was not administered, the patient went about immediately after the operation, and followed her usual avocations. In the other case, the patient had chloroform, and on this account chiefly she kept in bed that day; but the next day she was allowed to go out, and her cure was equally complete. In both cases the opening was large enough to admit the finger easily; and in one of them it was situate far in the vagina. The author recommended the use of many sutures, merely twisting them, and without either clamp or shot: he also advised that they should be allowed to remain some time to secure firm union, their presence occasioning no inconvenience. One of the cases cited was further remarkable inasmuch as, by the proof of sloughing which had previously taken place, no trace of the uterus could anywhere be discovered, and the patient has continued for some time past to menstruate through the bladder.

(c) CONCERNING DISEASES OF CHILDREN.

ART. 128.—*On Meningeal Apoplexy of Infants.*

By M. HERVIEUX, Physician to La Maternité, Paris.

(*Journ. de Méd. et Chir. Prat., Août, 1864.*)

Meningeal apoplexy is generally the result of difficult or tedious labour, but it may also originate in a haemorrhagic diathesis consequent on certain morbid states, amongst which M. Hervieux opines that sclerema and congenital or acquired debility are the most distinctly prominent. Fits of anger and exposure to the sun are also numbered by M. Bouchut amongst the causes of the disease, which they doubtless promote by inducing cerebral congestion.

Meningeal haemorrhage almost invariably occupies the great arachnoid cavity, where the blood, at first in a liquid state, soon coagulates. Legendre describes as follows the symptoms in new-born infants, and in children under three years; in none of the cases recorded by that much-lamented observer did the patients outlive this age:—

“ Vomiting may be absent, but generally occurs once or twice, when feverishness supervenes, accompanied by slight convulsive action of the ocular muscles, which usually leaves after it a certain degree of squinting. Loss of appetite and thirst are then noticeable; the motions are natural or easily induced. After a brief

interval the feet and hands are permanently contracted, and paroxysms of tonic or clonic convulsions soon set in. Consciousness and sensation are abolished during the fits, and the countenance assumes a darker hue. The intermissions are marked at first by a slight amount of drowsiness, which gradually turns to torpor as the disease progresses. Fever persists throughout, and daily increases in intensity, and the intervals between the paroxysms diminish in length, until at the close of the painful scene the fits run into each other in almost uninterrupted succession."

Valleix also includes violent convulsive attacks amongst the symptoms of the meningeal apoplexy of infants. He relates the history of a male child, who, two days after its birth, was brought to the Infirmary of the Foundling Hospital for contusion of the shoulder. The little patient was progressing most favourably, when, four days after his admission into the wards, he was suddenly seized with convulsions. The face became turgid, the neck and limbs rigid, the breathing laborious and fluttering. These symptoms lasted about thirty seconds, but recurred four or five times in a few minutes, when they subsided, and the child appeared to have perfectly recovered. Three hours afterwards, however, a fresh paroxysm supervened, and the infant expired in the course of three minutes. On dissection, copious sanguineous exudation, four or five lines in thickness, was found in the arachnoid cavity, with coagula adhering to the serous surface.

M. Hervieux opines that the symptoms most frequently coincident with meningeal haemorrhage in infants are the torpor and immobility of the little patient, and the feebleness of the scream, coupled with convulsions, especially of a tonic or permanent character. In doubtful cases the diagnosis derives greater certainty from the presence of the signs indicative of a haemorrhagic predisposition, such as the possible escape of sanguineous urine, the passage of blood with the faeces, or the oozing of froth tinged with blood from the mouth and nostrils. "We must not," says the author, "lose sight of the fact that, of all the morbid manifestations to which the haemorrhagic diathesis can give rise, meningeal apoplexy is unquestionably the most frequent."

The onset of the disease is sometimes sudden, and it runs rapidly through its stages, but occasionally assumes a slower progress, in the same manner as algid sclerema, gastro-enteritis, which usher in or pave the way to its development. Meningeal haemorrhage neonatorum varies in duration from a few hours to ten days or a fortnight, but seldom lasts beyond ten days. It usually proves fatal; but while informing the parents of the extreme gravity of the case, the practitioner must not remain inactive, but endeavour to evolve the indications of treatment from a careful inquiry into the causes of the haemorrhage.

In M. Hervieux's opinion, no other drink should be allowed but the nurse's or mother's milk, and all remedial agents should be applied externally. If algor be present, the child should be well wrapped up in cotton wool, or in his ordinary clothing covered with oiled silk, or india-rubber tissue: shampooing should be resorted to

for the dispersion of sclerema, and appropriate remedies be employed for the removal of thrush, enteritis, or general erythema. When torpor and convulsions supervene, leeches or cupping must be prescribed. One leech, for instance, should be applied behind each ear, so as to relieve, if possible, the congested state of the cerebrum. If the child is in a very debilitated condition, an interval of a few hours may be permitted to elapse between the application of the two leeches. When the muscles of the entire body are in a contracted state, pointing to haemorrhage in the envelopes of the spinal cord, cupping should be applied over the spine. Mustard poultices to the legs, and warm baths (94°), simultaneously with cold applications to the head, will also be found useful.

ART. 129.—*On the Treatment of Albuminuria in Children.*

By Dr. WILLIAM H. DICKINSON, Assistant-Physician
to the Hospital for Sick Children, London.

(*Edinburgh Medical Journal*, Sept., 1864.)

The granular kidney appears to be unknown in childhood. The only form of disease which produces albuminuria at this period of life is that which produces enlargement of the kidney and gives it a smooth mottled exterior. This is, in fact, a renal catarrh. The tubes become obstructed by an excess of their own epithelial growth, and hence arise all the evils of the disease. If only there is a free escape for the contents of the tubes, the vascularity of the gland will be relieved by secretion, and the disorder will soon be at an end. The principle of treatment must be to send as much water as possible through the organ. This fluid is devoid of irritating properties, and probably passes through the gland rather by filtration than true secretion. With these views the patients were restricted to a fluid diet. They took from two to four pints of distilled water daily, and small doses of the infusion of digitalis. When the active symptoms had subsided iron was given. Twenty-six cases are adduced in which this treatment had been pursued. Twenty-two recovered completely; three were lost sight of while improving, but while still having a small quantity of albumen in the urine; one case did badly, and eventually died under other treatment. Many of the cases were of great severity. These results appear better than those afforded by other methods. Among the in-patients at the Children's Hospital otherwise treated, eleven died out of thirty-nine; and of sixty-nine cases treated by Dr. Miller in dispensary practice, eight died. It was found on an average the little patients were restored to apparent health in thirty days, while fifteen days more were needed to get rid of the last traces of albumen. The use of the water did not seem in any case to increase the dropsy, but the contrary. It was usual, however, when the swelling was great to let the digitalis set up a certain amount of diuresis before ordering the full quantity. The subsequent use of iron is believed

to correct the effects of the disease, without influencing the disease itself. On the occurrence of secondary disorders, such as convulsions, or acute inflammatory attacks, it is argued that the treatment of the renal mischief should be sedulously persisted in, with such additions as might be called for. The anaemic state of brain in uræmic convulsions, and their frequent occurrence after the exhaustion of the diarrhoea or vomiting, are urged as reasons for abstaining from depressing remedies. A case is cited in which, under these circumstances, small doses of opium had been used successfully. A case is also given in which acute pleurisy passed off under the use of only local measures.

ART. 130.—*On Strophulus, or Tooth-rash.*

By M. HARDY, Physician to the Hôpital St. Louis, Paris.

(*Journ. de Méd et Chir. Prat.*, Jan., 1864.)

M. Hardy describes two varieties of this common and yet little known disease—the *simple* and the *pruriginous*.

Simple strophulus is frequent in children at the period of teething, and affects more especially the face; the eruption consists of red or whitish papulae, slightly acuminated, and sometimes complicated by the presence of erythema. This is an unimportant complaint, which requires interference only when it is attended with sufficient local irritation to oblige the infant to scratch the affected part, which induces oozing of a serous fluid, and the formation of yellowish scabs. In this case, M. Hardy prescribes diluents, baths, and starch or lycopodium powder externally. If necessary, laxatives may also be exhibited.

Pruriginous strophulus, which in M. Bazin's opinion is one of the manifestations of scrofula, is more interesting to the pathologist, and M. Hardy conceives it to be distinct in its nature from common tooth-rash, and often mistaken for prurigo and scabies.

The eruption is not confined to the face, but extends to the entire body, occupying more frequently the arms, where the papules, whether red or colourless, occasion an irritation which increases in the evening, and deprives the patient of his natural rest. M. Hardy lays much stress on what he views as the pathognomonic character of pruriginous strophulus, viz., the simultaneous presence of the peculiar lichen, together with that of pimples tipped with black, which belong to the order of prurigo. Between the latter eruption and pruriginous strophulus the discrimination is easy; but even experienced practitioners have mistaken at first sight the latter for scabies. In both affections the same complications, prurigo and ecthyma, are met with; and in addition, the local irritation is as great in one as in the other, and gives rise to violent scratching. One circumstance, however, establishes the difference, or at least supplies strong presumptive evidence in favour of strophulus, namely, the existence of papulae on the face. In itch this seldom occurs, but in strophulus the rash is very generally observed on this

part of the skin. Scabies, it is true, is not unfrequently accompanied by other eruptions, but they are to be found on the abdomen or the inner surface of the thighs (prurigo), or on the nates (ecthyma). These signs, confirmed by the discovery of the sulci of the acarus, will remove all doubt as to the true nature of the case.

M. Tranquebalme records in his thesis upwards of sixty cases, which show that strophulus seldom appears after adolescence. M. Hardy himself opines that it is a disease caused by poverty of blood, but he does not, like M. Bazin, connect it with the scrofulous diathesis.

According to M. Hardy, strophulus is not a contagious affection.

The gravity of the disease consists in its tendency to relapse if the patient does not alter the hygienic conditions which gave rise to it. The adulterated blood must be reconstituted, and the deleterious influences which unfavourably modified its component parts must be removed. It is therefore necessary to recommend change of residence, country air, and healthy exercise. If these, the most efficient measures, cannot be complied with, chalybeates, bark, and cod-liver oil should be prescribed; the local treatment should consist in alkaline or sulphurous baths, and as a sedative of the very distressing local irritation, the part affected should be dusted with the following powder, which M. Hardy also employs in zona:

B. Pulveris amyli, 3*ij*;

Zinci oxidi, 3*j*.

M.

R E P O R T S

ON THE

PROGRESS OF THE MEDICAL SCIENCES.

July—December, 1864.

THE intention of the following Reports is to pass in review the principal additions to each department of Medical Science which have been placed on record during the preceding six months. It is not contemplated that they should be confined exclusively to the notice of what is new; any fact or doctrine which may be considered practically useful will, although not strictly novel, be regarded as worthy of commemoration. It must be obvious to all who are aware of the immense mass of information which is almost daily put forth by the medical press of this and other countries, that the notice of every subject would be an impossibility. It therefore devolves upon the writers of each Report to select only such articles for retrospection as may possess superior recommendations, either of an intrinsic character, or in relation to the main end and aim of all medical knowledge—the alleviation of suffering and disease.

REPORT ON PHYSIOLOGY.

On the Coagulation of the Blood.

By Mr. JOSEPH LISTER, Professor of Surgery in the
University of Glasgow.

(*The Croonian Lecture, delivered before the Royal Society of London,
June, 1863.*)

There is no question in physiology at the present time of greater importance than the determination of the cause of the coagulation of the blood. It has always been a problem of great attraction to physiologists. It is unnecessary for us here to recite theories which have held possession of the schools, based chiefly on the conclusion of John Hunter, that "coagulation is an operation of life" due to the organization of its fibrin. Until the labours of Dr. Richardson it was felt that our knowledge was excessively deficient as to the changes which took place in the blood when coagulating, and their efficient cause. His researches brought out prominently and confidently a definite result—the evolution of ammonia; and to this he ascribed, with great clearness, the phenomenon of coagulation. This ammonia theory was supported by so many ingeniously-devised experiments, and, moreover, squared so well with the majority of observed phenomena in the coagulation of the blood, that it was eminently satisfactory to the minds of those who read the essay, and who rested content with its statements and conclusions. Since its publication, however, some competent observers have renewed their investigations, and directed especial attention to the ammonia theory. Amongst these counts Mr. Lister, one of the most acute and competent physiologists of the day, and especially known for his researches on inflammatory and other changes in the blood. The results at which Mr. Lister has arrived are, it should be observed, singularly opposed to the conclusions which Dr. Richardson has put forth as the fruit of his labours. Dr. Richardson, for instance, taught that blood shed into a vessel coagulated by virtue of throwing off its ammonia, and that the more rapid coagulation on agitation was in consequence of the more rapid loss of ammonia. Mr. Lister finds that agitation of the blood within its vessels, and without loss of ammonia, produces identical effects. Dr. Richardson attributed the acceleration of the coagulation of blood under heat to the volatilization of ammonia, and its retardation by cold to the imprisonment of that volatile alkali. Mr.

Lister finds heat and cold produce those relative effects equally when means are taken to place the blood in an ammonia bath.

The lecture of Professor Lister presents, in short, a series of experiments and conclusions totally adverse to the theory which Dr. Richardson propounded on the strength of his laborious research, and supports a physical explanation of the phenomena. What Mr. Lister shows is this—that the coagulation appears to be due to the influence exerted over blood by ordinary solids, which seem to have the power of attracting the coagulum. Thus a needle introduced through the wall of a sheep's vein, and left for twelve hours undisturbed, was found at the end of that time to have a crust of coagulum around it; and when blood is received into a vessel, coagulation is caused by the influence of the sides of the vessel, and the coagulation having thus commenced, travels inwards like advancing crystallization towards the centre of the mass. The vessels of the living body possess no peculiar action on the blood, and their lining membrane, in a state of health, is entirely negative in its relation to coagulation. But when the vessels are diseased, as for instance in arteritis and phlebitis, then the vessels act like ordinary solids, and give rise to coagulation in their interior:

The concluding remarks of the lecturer are these:—

"The real cause of the coagulation of the blood, when shed from the body, is the influence exerted upon it by ordinary matter, the contact of which for a very brief period effects a change in the blood, inducing a mutual reaction between its solid and fluid constituents, in which the corpuscles impart to the liquor sanguinis a disposition to coagulate. This reaction is probably simply chemical in its nature; yet its product, the fibrin, when mixed with blood-corpuscles in the form of an undisturbed coagulum, resembles healthy living tissues in being incapable of that catalytic action upon the blood which is effected by all ordinary solids, and also by the tissues themselves when deprived of their vital properties.

"These principles have, of course, very extensive applications to the study of diseases; but I must content myself with alluding very briefly to inflammation, the most important of all pathological conditions.

"If we inquire what is the great peculiarity of inflamed parts in relation to the blood as examined by the naked eye, we see that it consists in a tendency to induce coagulation in their vicinity—implying according to the conclusions just stated, that the affected tissues have lost for the time being their vital properties, and comport themselves like ordinary solids. Thus, when an artery or vein is inflamed, coagulation occurs upon its interior, in spite of the current of blood, precisely as would take place if it had been artificially deprived of its vital properties.—On one occasion I simulated the characteristic adherent clot of phlebitis by treating the jugular vein of a living sheep with caustic ammonia, and then allowing the circulation to go on through the vessel for a while, when, on slitting it up, I found its lining membrane studded with grains of pink fibrin, which could be detached only by scraping firmly with the edge of a knife. Again, comparing an inflammatory exudation into the peri-

cardium or into the interstices of the cellular tissue with dropsical effusions into the same situations, we are struck with the fact that, while the liquor sanguinis effused in dropsy remains fluid, the inflammatory product coagulates. Now we know that in intense inflammation the capillaries are choked more or less with accumulated blood-corpuses, which must cause great increase in the pressure of the blood upon their walls; and from what we know of the effect of venous obstruction in causing dropsical effusion of liquor sanguinis through increased pressure, we are sure that we have in the inflammatory state the physical conditions for a similar transudation of fluid through the walls of the capillaries. And the natural interpretation of the difference in the two cases as regards coagulation seems to be, that whereas in dropsy the fluid is forced through the pores of healthy vessels, in inflammation the capillary parietes have lost their healthy condition, and act like ordinary matter: so that the liquor sanguinis, having been subjected, immediately before effusion, to the combined influence of the injured tissue and the blood-corpuses, has acquired a disposition to coagulate, just like the buffy coat of horse's blood shed into a glass, or like the frog's liquor sanguinis filtered by Müller from its corpuscles, the injured vessels acting upon the blood like the filter.

"This view of the condition of intensely inflamed parts is exactly that to which I was led some years ago by a microscopic investigation, the results of which were detailed in a paper that received the honour of a place in the Philosophical Transactions. It was there shown, as I think I may venture to say, that the tissues generally are capable of being reduced under the action of irritants to a state quite distinct from death, but in which they are nevertheless temporarily deprived of all vital power, and that inflammatory congestion is due to the blood-corpuses acquiring adhesiveness such as they have outside the body, in consequence of the irritated tissues acting towards them like ordinary solids."

On Coagulation of the Blood.

By Mr. GULLIVER.

(*Medical Times and Gazette*, October 17, 1863.)

In one of his lectures delivered last year at the Royal College of Surgeons of England, Mr. Gulliver made the following remarks upon this subject:—

"Finally, although we have so clearly shown that an addition of red corpuscles favours or hastens the coagulation of the blood, we must not conclude that they are either the efficient cause, or essential predisposing agent, of that coagulation. In truth, that cause remains yet so mysterious that we may well be thankful to those physiologists who, like Dr. Richardson and Dr. Davy, devote their talents to this important inquiry. We have shown in a former lecture that vegetable juices will coagulate when let out of the plant quite independently of the presence of corpuscles or cells; and

that two varieties of animal serum, destitute of any corpuscles, may coagulate when mixed together, while no coagulation will ever take place in either of these fluids separately. Above all, in reference to the vital doctrine of Mr. Hunter, and the chemical one of Dr. Richardson—those fluids could scarcely be supposed to acquire life, merely by the operation of that mixture, and we have demonstrated that fibrin may be kept fluid for an indefinite time in pickle and yet retain its coagulable property; and it has been plainly proved by Dr. Davy's and my own experiments, that the coagulation of animal and vegetable fibrin may take place without the emission of any ammonia whatever. Besides, while fully admitting the predisposing effect both of the corpuscles and of extraneous or dead matter, these cannot be the central or fundamental cause of the coagulation, since we have seen that the serum which coagulated and that which would not coagulate were alike exposed in open glass jars to the air; and such truly dead and heterogeneous things as ice and alkaline salts preserve the fluidity and coagulable power of that very blood which will coagulate readily on the addition to it either of heat (mere thawing) or pure water; nay, even lumps of ice and crystal of salts, as I have often proved, may be added to specimens of such blood without causing its coagulation until the required condition, either of warmth or dilution, has been complied with; in short, every one of the causes, as far as I know, hitherto propounded as the essential efficient of the coagulation of the blood, has been tried and found wanting."

On Fibrin and the Cause of its Coagulation.

By DR. A. SCHMIDT.

(*Archiv f. Anat. u. Physiol.*, 1861; and *Med.-Chir. Rev.*, July, 1863.)

Schmidt's very interesting researches show that is principally the cells which contain the active principle in the process of coagulation. Lymph and chyle do not possess the property of spontaneous coagulation before their admixture with cells, which takes place in the lymphatic glands. But if cells are artificially added to pure lymph and chyle, coagulation begins at once. The difference in the appearance of fibrinous coagula was found to depend, not on a variety of fibrinous substances, but on the difference of the cells which produce the coagulation. Serous effusions coagulated when to their clear fluid a small quantity of defibrinated blood was added. The action of the active principle is not analogous to that of ferment, for it is used up in the process. It combines chemically with the coagulable substance. Schmidt calls those substances which have the power of inducing certain albuminous fluids to coagulate *fibrino-plastic*. Among these are especially blood-lymph, chyle, and pus-corpuscles; but also the cornea, the watery extract of the crystalline lens, the humours of the eye, saliva, &c. The fluids which contain the coagulable principle in solution he names *fibrino-genous*. The fibrino-plastic properties he further shows to depend on a substance which is identical with haemato-globuline. As regards the influence

which the gases of the blood exert upon its coagulation, he makes out that the presence of carbonic acid always retarded coagulation. This retarding influence is but very slight when exerted on fresh blood. He thinks that the differences in time which arterial and venous blood show in reference to coagulation may be accounted for by the different amounts of carbonic acid which they respectively contain. Oxygen and atmospheric air he found to have no direct influence in promoting coagulation; but when much carbonic acid is contained in the blood, oxygen promotes coagulation by displacing that gas. Blood which in vacuo had been freed of all its gases did not lose its fibrino-plastic power. Contact with animal tissues retards coagulation.

On the Influence of Physical and Chemical Agents upon Blood; with special reference to the Mutual Action of the Blood and the Respiratory Gases.

By DR. GEORGE HARLEY, Professor of Medicine and Jurisprudence in University College, London.

(*Proceedings of the Royal Society, May 10, 1864.*)

This communication is divided into two parts. The first is devoted to the investigation of the influence of certain physical agencies, viz., simple diffusion, motion, and temperature, and of the conditions of time and the age of the blood itself. The second part includes the consideration of the influence of chemical agents, especially such as are usually regarded as powerful poisons.

The paper commences with a description of the apparatus employed, and the method followed in conducting the inquiry; and the details of the several experiments are then given. The following is a brief statement of the results.

PART I.

1. The experiments on diffusion showed that venous blood not only yields a much greater amount of carbonic acid than arterial blood, but also absorbs and combines with a larger proportion of oxygen.

2. Motion of the blood was found to increase the chemical changes arising from the mutual action of the blood and the respiratory gases.

3. The results of the experiment on the influence of time led to the conclusion that the blood and air reciprocally act on each other in the same way out of the body as they do within it, and that their action is not instantaneous, but gradual.

4. It was ascertained that a certain degree of heat was absolutely essential to the chemical transformations and decompositions upon which the interchange of the respiratory gases depends. The higher the temperature up to that of 38° C. (the animal heat), the more rapid and more effectual were the respiratory changes; whereas a temperature of 0° C. was found totally to arrest them.

5. The influence of age on the blood was found to be very marked,

especially on its relation to oxygen. The older and more putrid the blood becomes, the greater is the amount of oxygen that disappears from the air; and although at the same time the exhalation of carbonic acid progressively increases with the age of the blood, yet its proportion is exceedingly small when compared with the large amount of oxygen absorbed.

6. The average amount of urea in fresh sheep's blood was ascertained to be 0·559 per cent., and its disappearance from the blood during the putrefactive process was very gradual, there being as much as 0·387 per cent. in blood after it was 304 hours old.

PART II.

The chemical agents employed were animal and vegetable products and mineral substances.

1. The effect of snake-poison was found to be an acceleration of the transformations and decompositions occurring in blood, upon which the absorption of oxygen and the exhalation of carbonic acid depend.

2. The presence of an abnormal amount of uric acid in blood was also found to hasten the chemical changes upon which the absorption of oxygen and exhalation of carbonic acid depend.

3. Animal sugar, contrary to what had been anticipated, retarded the respiratory changes produced in atmospheric air by blood.

4. The influence of hydrocyanic acid was studied both upon ox-blood and human blood, and found to be the same in each case, namely, to arrest respiratory changes.

5. Nicotine was also found to diminish the power of the blood either to take up oxygen or give off carbonic acid gas and thereby become fitted for the purposes of nutrition.

6. The effect of woorara poison, both on the blood in the body and out of it, was ascertained to be in some respects similar to that of snake-poison, namely, to increase the chemical decompositions and transformations upon which the exhalation of carbonic acid depends; but differed in retarding, instead of hastening, the oxidation of the constituents of the blood.

7. Antiar poison and aconite were found to act alike, inasmuch as both of them hastened oxidation and retarded the changes upon which the exhalation of carbonic acid depends; in both respects offering a striking contrast to woorara poison, which, as has just been said, diminishes oxidation and increases the exhalation of carbonic acid gas.

8. The effect of strychnine on the blood, both in and out of the body, was studied, and found to be in both cases identical, namely, like some of the other substances previously mentioned, to arrest respiratory changes. Moreover, in one experiment in which the air expired from the lungs of an animal dying from the effects of the poison was examined, it was found that the arrest in the interchange of the gases too, caused the animal to die.

9. Brucine acts in a manner similar to strychnine, but in a much less marked degree.

10. Quinine also possesses the power of retarding oxidation of the blood, as well as the elimination of carbonic acid gas.

11. Morphine has a more powerful effect in diminishing the exhalation of carbonic acid gas, as well as the chemical changes upon which the absorption of oxygen by blood depends.

Under this head the effects of anaesthetics upon blood are next detailed; and, in the first place, the visible effects of chloroform upon blood are thus described:—If 5 or more per cent. of chloroform be added to blood, and the mixture be agitated with air, it rapidly assumes a brilliant scarlet hue, which is much brighter than the normal arterial tint, and is, besides, much more permanent. When the mixture is left in repose, it gradually solidifies into a red-paint-like mass, which when examined under the microscope is frequently found to contain numerous prismatic crystals of an organic nature. If the blood of an animal poisoned from the inhalation of chloroform be employed in this experiment, the paint-like mass will be found to be composed in greater part of the crystals just spoken of; the crystals in this case being both larger and finer than when healthy blood is employed. Chloroform only partially destroys the blood-corpuscles. Its chemical action is to diminish the power of the constituents of the blood to unite with oxygen and give off carbonic acid.

The action of sulphuric ether upon blood differs in many respects from that of chloroform. In the first place, ether has a powerful effect in destroying the blood-corpuscles, dissolving the cell-walls and setting the contents free. In the second place, ether prevents the blood from assuming an arterial tint when agitated with air. The higher the percentage of the agent, the more marked the effect. In the third place, ether neither diminishes the absorption of oxygen nor the exhalation of carbonic acid by blood; and lastly, it has a much more powerful effect in causing the constituents of the blood to crystallize. For example, if an equal part of ether be added to the blood of a dog poisoned by the inhalation of chloroform, as the ether evaporates groups of large needle-shaped crystals are formed. Under the microscope the crystals are found to be of a red colour and prismatic shape.

Alcohol acts upon blood somewhat like chloroform; it arrests the chemical changes, but in a less marked degree.

Amylene was found to act like ether upon blood, in so far as it did not diminish the absorption of oxygen or retard the elimination of carbonic acid. It differed, however, from ether in not destroying the blood-corpuscles.

In the last place, the action of mineral substances is stated, viz:—

1. Corrosive sublimate was found to increase the chemical changes which develop carbonic acid, and to have scarcely any effect on those depending upon oxidation; its influence, if any, is rather to diminish them than otherwise.

2. Arsenic seems to retard both the oxidation of the constituents of the blood and the exhalation of carbonic acid.

3. Tartrate of antimony increases the exhalation of carbonic acid gas, while it at the same time diminishes the absorption of oxygen.

4. Sulphate of zinc and sulphate of copper both act like tartrate of antimony, but not nearly so powerfully.

Lastly, phosphoric acid was found to have the effect of increasing the chemical transformations and decompositions upon which the exhalation of carbonic acid depends.

On the Colour of the Blood.

By Professor GULLIVER.

(*Medical Times and Gazette*, Jan. 3, 1863.)

In his lectures on the blood of vertebrata, delivered at the Royal College of Surgeons of England, during the session 1861-62, Professor Gulliver makes the following important remarks on this subject:—

“ *Colour of the Blood.*—We now proceed to consider the effects of the red corpuscles on the colour of the blood, as introductory to the interesting inquiry as to their agency in vivifying it and the parts through which they circulate, as well as their use in connexion with respiration and the production of animal heat. When the corpuscles are seen singly as transparent objects under the microscope, they are of a pale straw colour, of a redder hue when a few of them are so aggregated as to allow of less light passing through them, and of a darker colour when still more are clumped together. Newton observed that blood is one of those fluids which is yellow when viewed in very thin slices. To the naked eye the colour is so well known, that ‘as red as blood’ is a popular phrase; and no physiologist can be ignorant that this colour is owing, in vertebrates, to the red corpuscles, and is of a bright scarlet in arterial blood. But the precise causes of this difference of colour, and the circumstances under which it increases, diminishes, or even ceases, in the healthy animal, are by no means so well known.

“ *Discordant Observations on the Colour of Arterial and Venous Blood reconciled by Effects of Temperature.*—In the first place, as to the effect of temperature. The difference of colour is most marked in very cold weather, and least so in very hot weather; but having been always taught the striking floridness of arterial blood, and the darkness of venous blood, and having commonly witnessed this difference of colour in our own temperate climate, we seldom or never think of the possibility of any doubt or question as to this subject. Yet there have been very discordant observations, and by some of our best physiologists, as to the difference of colour between arterial and venous blood; and as the question is an important one, and even connected with the practice of our Profession, a few historical notices will be well calculated to fix this interesting point on our attention. Harvey believed that the colour of the two kinds of blood is essentially the same, though he admitted that there might be an accidental difference in the colour of the blood as it flows from the artery or vein of a living animal. But the question was a subject of controversy fifty years ago. Lower specially proved the

difference of colour, and correctly inferred, from experimental inquiry, that the change of colour is produced in the lungs; and further supported this inference by observing that air produces just the same florid hue on the surface of the blood-clot out of the body—an observation which had also been made two or three years before, by Fracassati, in a paper published in the *Philosophical Transactions*. Mayow fully admitted Lower's conclusions, and attributed the change of colour in the blood, during its course through the lungs, to its abstracting from the atmosphere a 'nitro-aërial spirit,' many of the properties of which, as he describes them, belong to oxygen. Dr. William Hunter, in his Lectures, as early as 1759, taught the true difference of colour between arterial and venous blood. Yet we find Haller, in 1756 and 1780, opposing the correct conclusions of Lower, and even asserting, that a hundred observations in dogs had convinced him that there is no difference of colour between the blood of the pulmonary artery and the blood of the pulmonary vein. But Priestley, who discovered his dephlogisticated air—our oxygen—in August, 1774, ascertained the correctness of Lower's results, and that air will act on the blood through a moist membrane; and he also proved that the brightened colour of the blood is produced only by the oxygen, and that carbonic acid, hydrogen, and azote have a contrary effect. Mr. Hunter removed the sternum from the dog, kept up the breathing of the animal by a bellows, and saw the blood acquire the scarlet colour in passing through the lungs. Then, at last, we have Dr. Davy making observations on sheep, at Malta, in the summer of 1829, when he carefully compared the blood of the jugular vein and of the carotid artery, and could see no difference whatever in their colour; in each it was less florid than the arterial blood of the same animal in an English winter, and less dark than the venous blood, being of a hue between the two. And this observation was so carefully conducted, that there could have been no mistake. In explanation, he supposes, that the higher the atmospheric temperature, and, consequently, the less need for the production of heat within the animal, the less difference there will be between arterial and venous blood, and the less power the venous blood will have of combining with oxygen, and of forming or evolving carbonic acid. Now, it is remarkable that Dr. Crawford's experiments, upwards of fifty years before, lead to the same opinion. He put a dog for more than half-an-hour into warm water, and then found it difficult to distinguish the arterial from venous blood.

"Here, then, is a series of facts not without value either in a physiological or in a practical point of view. The former has just been mentioned: and, as to the latter, we must be very cautious in judging from the colour alone during hot weather, when the temperature of the atmosphere is upwards of 80° , of whether blood flowing from a wound be arterial or venous. Very likely Harvey's experiments were made at the hot season in Italy, and that Haller's took place during a warm summer at Lausanne.

"*Causes of the Bright and Dark Colour of Blood.*—And now, having discussed the effect of temperature on the colour of the blood in the living body, we come to consider by what agent and

how this effect is produced in the blood both in and out of the body. It is well known that blood is darkened even by a very brief stagnation both in living arteries and veins; and I have often been amused at the trick by which scheming farriers show 'the blood as black as pitch,' in order to prove their sagacity in having let out 'that bad blood.' No doubt, the general opinion, that the florid colour of arterial blood is due to the action of oxygen, according to the old view of Mayow, confirmed a hundred years afterwards by Priestley, is the correct one; but, in 1835, this was controverted very ingeniously by Dr. Stephens. It had been long known that earthy and alkaline neutral salts will render dark venous blood florid; and he was led to the conclusion, that the florid colour of arterial blood is caused by the agency of the salts of the serum on the hematozine, and that oxygen changes the colour of blood from venous to arterial merely by removing the carbonic acid, which, we have already remarked, Dr. Priestley had long before proved to darken the colour of the blood, and Dr. Stephens considers as the cause of this dark hue in venous blood. These views were adopted by Dr. Turner in his work on Chemistry, and Mr. Hoffman was also favourable to them. Dr. Christison agitated atmospheric air with a mixture of serum and red corpuscles of blood, and always found that oxygen disappeared, while carbonic acid was produced, but, owing to the strong solvent power of serum on this acid, he believed that more of it was formed than appeared in the residual air. In all these experiments venous blood acquired a bright vermillion hue, and the florid colour of arterial blood was heightened. Dr. Davy obtained the same results as to the absorption of oxygen by the blood, and the consequent brightening of its colour; but in the residual air he could detect only a trace of carbonic acid, and none at all when, instead of atmospheric air, pure oxygen was agitated with the blood. He further observed that venous blood, when subjected to the air-pump, did not acquire the arterial hue, even when carbonic acid was extracted; and yet that the florid colour was imparted to venous blood by agitating it with a mixture of oxygen and carbonic acid gases, although the blood certainly absorbed a much larger portion of the carbonic acid than of the oxygen.

"These results are quite irreconcilable with the doctrine of Dr. Stephens, but agree well with the older views which he disputes. Dr. C. J. B. Williams gave some experiments to prove that the florid effect produced by oxygen and the salts is by their causing more light to be reflected through the colouring matter. Dr. Davy concludes that neutral salts brighten the blood by so separating the corpuscles that they reflect more light; that water, acids, and other agents darken the blood by altering the form of the corpuscles and partially dissolving the colouring matter; and that hematozine is black only in mass, and red when powdered, or when viewed in a small portion by transmitted light. My own observations are to the same effect generally. I found, after Nasse, Scherer, and Mulder, that agents, as pure water and carbonic acid, which darken the blood, make the corpuscles tumid or globular, and that florid arterial blood soon became dark coloured when kept in a jar, and

remarkably so when putrefaction began; and the form of the red corpuscles was more or less altered, when the true bright vermillion hue of arterial blood could no longer be restored in it either by oxygen or neutral salts. The effect, too, of these salts in fresh blood is always to separate the corpuscles, and render them slightly thinner, smaller, or more compact, which oxygen produces also in a less degree: nor could I find that either neutral salts, sugar, or oxygen, would strike a scarlet colour with pure and nearly black hematozine.

" But the valuable observations of Dr. Wells, published in the *Philosophical Transactions* of 1797, and strangely neglected, notwithstanding Dr. Davy's notice of them in 1838, are so conclusive on this subject, that much discussion might have been spared had Wells' paper not been forgotten. He obtained the same results as those just mentioned, as to the action of air and neutral salts not brightening the colour of pure hematozine—that nitre does not change the colour of a solution of hematozine, and proved by an ingenious experiment that the opacity of blood, and the reflection of light from it, are increased by neutral salts; and observed that these salts and air—of course the oxygen—affect the blood, just as bright vermillion is produced from dark cinnabar by subjecting it to minute mechanical division. Thus, then, the result is, that it is not simply to chemical agency, but either to changes in the form of the corpuscles, or to their more or less state of aggregation, so as to dispose them either to reflect or to absorb more or less light, that the effect of many substances on the colour of the blood is owing; and we have already specially stated how neutral salts and oxygen act in this respect. Nor can we dismiss the observations of this eminent man, made upwards of sixty years since, without remarking the grievous injustice to him and to English science, by our translators and commentators adopting the sorry claim of very recent German genius to 'the discovery that everything relating to the colour of the blood does not pertain to chemistry!'

" If you look again at the diagrams, you will see that the red corpuscles, when highly magnified and viewed by transmitted light, appear of a pale straw colour, and that they are of a deep red hue only when seen in mass, and are, consequently, more or less opaque. The extension or diffusion of their surface, and of the light through them, under the microscope, are just what might be expected from a consideration of chromatic laws. Hematozine only appears black in mass; in small transparent portions and in powder it is red. Now, it is well known that the more concentrated the colouring-matter the darker it is, and the more diffused or diluted, *cæteris paribus*, the lighter it becomes. The more, also, the colour is mixed with white or reflected light, and the purer the white ground on which the colour is spread, so much the brighter is the effect. Indeed, the great masters of colouring of the Venetian school practised their consummate art on these beautifully simple principles. The colours they used were transparent, not black; and darkness or blackness was produced rather by a concentration of colour than by a direct use of any black pigment."

On the Electricity of the Blood.

By M. SCOUTETTEN.

(Comptes Rendus, Tome lvii. pp. 327 and 373.)

The author's researches were undertaken with the view of proving the existence, and of determining the character, of the electric reaction of red and black blood. Special precautions were taken in order to show that the electricity was in the blood itself. The experiments were carried on with horses. The right carotid and left jugular vein were exposed and carefully isolated, and after two loose ligatures had been placed on each vessel at a distance of about twelve centimètres from each other, the portion of vessel inclosed between the ligature was incised, so as to allow the escape of the small quantity of blood contained in it. The next step consisted in the introduction, into each vessel, of a glass tube, open at each end, and containing in its interior a sheet of platinum, measuring ten square centimètres, and folded up like a fan. To this was joined a platinum wire, half a centimetre in diameter, and twenty-five centimètres long, covered with gutta percha, except at its free extremity, which could be connected by means of an insulated coil with an excellent galvanometer of Nobili's.

The coats of the vessels were then divided completely so as thoroughly to isolate the tubes and to remove all suspicion of any currents being transmitted along the coats of the vessels. The tubes were of course maintained *in situ* by ligatures. These precautions having been taken, and the needle of the galvanometer standing at zero, the blood was allowed to pass through the tubes by removing the loose ligatures first applied beyond the incision in the vessels. On closing the circuit, the needle immediately showed the existence in the arterial blood of a positive current; in other words, the direction of the inner current was *from* the venous blood *towards* the arterial. In the 1st experiment, movements of the horse having disturbed the apparatus, the amount of the deflexion of the needle could not be ascertained. In the 2nd experiment, the needle was seen to point to 55° , and in the 3rd it rested opposite $+50^{\circ}$.

The 4th experiment was made differently. The horse was bled simultaneously from the right carotid and the left jugular, and the blood made to flow into two separate porous cells. On immersing into them two platinum electrodes, very powerful reaction instantly followed, and the needle after a time came to a standstill opposite 75° , keeping that position for 10'. When coagulation—not decomposition—of the blood had taken place, the needle still pointed to 70° . The direction of the current was the same as in the previous experiments, namely, from the black to the red blood.

From these researches, the author draws the following conclusions:—

Since it has been demonstrated that red and black blood when in contact, through the coats of vessels, which act like porous cells, give rise to electric reactions shown by the galvanometer, it must

be admitted that all parts of our body being traversed by the blood, there must of necessity be a constant discharge of electricity even in the most delicate tissues. 2ndly. That every organic molecule is unceasingly stimulated by the escaping electric fluid, and that it is principally under the influence of this constant excitation that all our functions are performed. Thus the oxygen contained in red blood burns the organic molecules with which it comes in contact, and produces heat. Again, during digestion, it is under the influence of electricity that the process of election of nutritive molecules, and afterwards assimilation, is carried on. The same obtains with respiration, and with internal and external secretions; in a word, with all our functions, however simple or complicated they may be. Electricity is the prime mover of all organic processes, and when it ceases all comes to a stop. Furthermore, this disengaged electricity is instantly recomposed, and there is no escape of free electricity from the body.

The phenomena are in perfect accordance with the electric phenomena developed during combustion; for it is well known that during the latter process charcoal becomes charged with negative electricity, and the surrounding atmosphere with positive electricity, or, more exactly, a current passes from the charcoal to the oxygen of the air.* Now, the principal effect of red blood is to produce actual combustion in our tissues, on account of the oxygen it contains.

*On Electricity as the Principle which causes the Vitality
and Coagulating Property of the Blood.*

By Dr. R. C. SHETTLE.

(*Lancet*, August 27 and September 12, 1863.)

The electricity which Dr. Shettle believes to be present in arterial blood is regarded as the cause of the transformation of arterial into venous blood. Of the experiments which show the presence of electricity in arterial blood and its absence in venous blood, this is one:—

“ *Experiment.*—The jugular vein of a dog (under the influence of chloroform) having been secured by means of two ligatures, was divided, and a glass tube inserted into the upper portion. Blood was then drawn into a clean glass vessel, taking care to avoid exposure to air as much as possible. A cork, in which was placed two platinum electrodes, covered with folds of blotting-paper and bladder, and upon which electrodes some pains had been bestowed to render them homogeneous, was then accurately fitted into the mouth of the vessel, and the wires of the electrodes applied at once to the galvanometer. The same alteration of the needle immediately followed that had succeeded the immersion of the plates into water—viz., a diver-

* Gaugain, On the Development of Electricity during Combustion—*Comptes Rendus de l'Acad. des Sciences*, tom. xxxviii, p. 731. Paris, 1854.

gence of 45° to the negative, the needle falling back again directly to zero, where, after a slight vibration or two, it remained, showing, as far as the galvanometer was capable, the absence of electricity in the fluid vessel.

"The carotid artery of the same dog was then tied, and treated in the same way as the vein, blood being drawn into a glass vessel similar to that used for the venous blood. Electrodes in every respect precisely similar, and which also had been tested immediately before using, were inserted as before, and the wires again attached to the galvanometer. Thirty degrees of positive electricity were at once shown, and the needle was two hours and fifteen minutes returning to zero, the backward motion being very regular. The electrodes were allowed to remain in the arterial blood six hours before being again examined, when a perfectly distinct line marked the depth to which the electrodes had been immersed, the upper portion being of a very dark venous, and the lower portion remaining of a bright arterial hue. The clot was then removed, and found to be almost black, and not firmly coagulated.

"I believe this experiment is sufficient to show that the abstraction of electricity from the blood causes it to lose its arterial character, certainly with regard to colour, and, if so, must it not also be attended with loss of its stimulating property?"

Speaking of the influence of electricity in determining the coagulation of the blood, Dr. Shettle says:—

"In a previous experiment, blood was drawn from the vein and artery of a dog, likewise under the influence of chloroform, into clean vessels. Galvanic circuits, consisting of clean plates of copper and zinc, were introduced through corks which were accurately adjusted to the mouths of the vessels so as to exclude air. Coagulation in each instance proceeded *round the plates, especially the zincode*. On the second day a peculiar formation appeared proceeding from the zincode to the side of the vessel, which might be compared not inaptly to the growth of a fibrous clot. This formation in the venous blood was loose, and the cellular spaces between the fibres large; but in the arterial, the growth was firm in texture and smaller in size as compared with the venous. This process was allowed to go on four days, when it had apparently ceased to increase. The vessels were then carefully opened; but, owing to the plates being attached to the corks, this could not be done without disturbing the formation above alluded to, and which at once became broken up, and sank to the bottom of the vessel, presenting under the microscope granular cells. The clot in both instances occupied not only the space between the plates, but also encrusted the zinc plate with a very firm coagulum; while on the side of the copper plate most distant from the zinc, it was scarcely to be called a fibrous clot, having more the appearance of transparent jelly. The clot taken from the zinc pole in the arterial blood had become as hard as a piece of dried leather, and, after exposure to the air, could be broken. That the coagulation of the blood is due to the action of a current of electricity, cannot, I think, be reasonably doubted, inasmuch as I have shown that electricity does exist in arterial blood;

and although I have not been able to detect it in venous blood, it is possible that it may exist in very small proportion, and so account for the very feeble coagulation of venous blood when perfectly excluded from the air."

On the Identity of Hæmatoidin and Bilifulvin.

By Dr. MAX JAFFE, of Berlin.

(*Virchow's Archiv*, Bd. xxiii., 1861; and *Medico-Chirurgical Review*, October, 1862.)

Dr. Jaffe, after alluding to the researches of Zencker, Brücke, Valentine, Kühne, and others, on the same subject, states that he has obtained crystals in a chloroform solution of bile, which in form and all known reactions closely agree with hæmatoidin. He then proceeds to describe an examination of a cerebral apoplectic cicatrix, which proved that hæmatoidin and bilifulvin were identical.

This cicatrix, which was of a yellowish-brown colour, and showed under the microscope a large number of crystals of hæmatoidin, was dried in a water-bath and cut into small pieces. A chloroform extract was made, and was then moistened with a few drops of absolute alcohol, by which the action of the chloroform was apparently facilitated. The extract which contained the cerebral fat was of a deep yellow colour. The chloroform extract was then gently evaporated down in a watch-glass, and for twenty-four hours was placed in a darkened place (for fear of oxidation changes, which, in the case of bilifulvin solutions, take place in the sunlight, and quickly produce changes of colour), and then examined microscopically. It was found to consist of transparent, golden-yellow, beautifully formed crystals, corresponding accurately to hæmatoidin in form.

On freeing the crystals by ether from fat, a portion of them became dissolved therein (pure ether as well as pure alcohol partially dissolves bilifulvin), the remaining crystals being soluble with tolerable facility in solution of carbonate of soda. The yellow solution became green during filtration. A small remaining portion was treated with sulphuric acid and examined, and the crystals then showed the well-known colour-play of biliary colouring-matter. Similar changes were observed in the chloroform solution on the addition of sulphuric acid.

From these observations on the microscopical and chemical character of the crystals, the author concludes that hæmatoidin and bilifulvin are identical, and he points out the bearing which this statement has upon our views regarding the changes of blood-cells in the liver.

Examinations of other apoplectic changes in portions of brain which had been long immersed in spirit did not disclose crystals of bilifulvin.

The fat which remained after distillation of the yellow chloroform extract assumed the shape of margaric acid crystals, and probably mechanically hindered the formation of bilifulvin crystals.

especially on its relation to oxygen. The older and more putrid the blood becomes, the greater is the amount of oxygen that disappears from the air; and although at the same time the exhalation of carbonic acid progressively increases with the age of the blood, yet its proportion is exceedingly small when compared with the large amount of oxygen absorbed.

6. The average amount of urea in fresh sheep's blood was ascertained to be 0·559 per cent., and its disappearance from the blood during the putrefactive process was very gradual, there being as much as 0·387 per cent. in blood after it was 304 hours old.

PART II.

The chemical agents employed were animal and vegetable products and mineral substances.

1. The effect of snake-poison was found to be an acceleration of the transformations and decompositions occurring in blood, upon which the absorption of oxygen and the exhalation of carbonic acid depend.

2. The presence of an abnormal amount of uric acid in blood was also found to hasten the chemical changes upon which the absorption of oxygen and exhalation of carbonic acid depend.

3. Animal sugar, contrary to what had been anticipated, retarded the respiratory changes produced in atmospheric air by blood.

4. The influence of hydrocyanic acid was studied both upon ox-blood and human blood, and found to be the same in each case, namely, to arrest respiratory changes.

5. Nicotine was also found to diminish the power of the blood either to take up oxygen or give off carbonic acid gas and thereby become fitted for the purposes of nutrition.

6. The effect of woorara poison, both on the blood in the body and out of it, was ascertained to be in some respects similar to that of snake-poison, namely, to increase the chemical decompositions and transformations upon which the exhalation of carbonic acid depends; but differed in retarding, instead of hastening, the oxidation of the constituents of the blood.

7. Antiar poison and aconite were found to act alike, inasmuch as both of them hastened oxidation and retarded the changes upon which the exhalation of carbonic acid depends; in both respects offering a striking contrast to woorara poison, which, as has just been said, diminishes oxidation and increases the exhalation of carbonic acid gas.

8. The effect of strychnine on the blood, both in and out of the body, was studied, and found to be in both cases identical, namely, like some of the other substances previously mentioned, to arrest respiratory changes. Moreover, in one experiment in which the air expired from the lungs of an animal dying from the effects of the poison was examined, it was ascertained that the arrest in the interchange of the gases took place before the animal was dead.

9. Brucine acts in a similar manner to strychnine, but in a much less marked degree.

10. Quinine also possesses the power of retarding oxidation of the blood, as well as the elimination of carbonic acid gas.

11. Morphine has a more powerful effect in diminishing the exhalation of carbonic acid gas, as well as the chemical changes upon which the absorption of oxygen by blood depends.

Under this head the effects of anaesthetics upon blood are next detailed; and, in the first place, the visible effects of chloroform upon blood are thus described:—If 5 or more per cent. of chloroform be added to blood, and the mixture be agitated with air, it rapidly assumes a brilliant scarlet hue, which is much brighter than the normal arterial tint, and is, besides, much more permanent. When the mixture is left in repose, it gradually solidifies into a red-paint-like mass, which when examined under the microscope is frequently found to contain numerous prismatic crystals of an organic nature. If the blood of an animal poisoned from the inhalation of chloroform be employed in this experiment, the paint-like mass will be found to be composed in greater part of the crystals just spoken of; the crystals in this case being both larger and finer than when healthy blood is employed. Chloroform only partially destroys the blood-corpuscles. Its chemical action is to diminish the power of the constituents of the blood to unite with oxygen and give off carbonic acid.

The action of sulphuric ether upon blood differs in many respects from that of chloroform. In the first place, ether has a powerful effect in destroying the blood-corpuscles, dissolving the cell-walls and setting the contents free. In the second place, ether prevents the blood from assuming an arterial tint when agitated with air. The higher the per-cent-age of the agent, the more marked the effect. In the third place, ether neither diminishes the absorption of oxygen nor the exhalation of carbonic acid by blood; and lastly, it has a much more powerful effect in causing the constituents of the blood to crystallize. For example, if an equal part of ether be added to the blood of a dog poisoned by the inhalation of chloroform, as the ether evaporates groups of large needle-shaped crystals are formed. Under the microscope the crystals are found to be of a red colour and prismatic shape.

Alcohol acts upon blood somewhat like chloroform; it arrests the chemical changes, but in a less marked degree.

Amylene was found to act like ether upon blood, in so far as it did not diminish the absorption of oxygen or retard the elimination of carbonic acid. It differed, however, from ether in not destroying the blood-corpuscles.

In the last place, the action of mineral substances is stated, viz:—

1. Corrosive sublimate was found to increase the chemical changes which develope carbonic acid, and to have scarcely any effect on those depending upon oxidation; its influence, if any, is rather to diminish them than otherwise.

2. Arsenic seems to retard both the oxidation of the constituents of the blood and the exhalation of carbonic acid.

3. Tartrate of antimony increases the exhalation of carbonic acid gas, while it at the same time diminishes the absorption of oxygen.

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(Comptes Rendus, Tome lvii. pp. 327 and 373.)

The author's researches were undertaken with the view of proving the existence, and of determining the character, of the electric reaction of red and black blood. Special precautions were taken in order to show that the electricity was in the blood itself. The experiments were carried on with horses. The right carotid and left jugular vein were exposed and carefully isolated, and after two loose ligatures had been placed on each vessel at a distance of about twelve centimètres from each other, the portion of vessel inclosed between the ligature was incised, so as to allow the escape of the small quantity of blood contained in it. The next step consisted in the introduction, into each vessel, of a glass tube, open at each end, and containing in its interior a sheet of platinum, measuring ten square centimètres, and folded up like a fan. To this was joined a platinum wire, half a centimètre in diameter, and twenty-five centimètres long, covered with gutta percha, except at its free extremity, which could be connected by means of an insulated coil with an excellent galvanometer of Nobili's.

The coats of the vessels were then divided completely so as thoroughly to isolate the tubes and to remove all suspicion of any currents being transmitted along the coats of the vessels. The tubes were of course maintained *in situ* by ligatures. These precautions having been taken, and the needle of the galvanometer standing at zero, the blood was allowed to pass through the tubes by removing the loose ligatures first applied beyond the incision in the vessels. On closing the circuit, the needle immediately showed the existence in the arterial blood of a positive current; in other words, the direction of the inner current was *from the venous blood towards the arterial*. In the 1st experiment, movements of the horse having disturbed the apparatus, the amount of the deflexion of the needle could not be ascertained. In the 2nd experiment, the needle was seen to point to 55° , and in the 3rd it rested opposite $+50^\circ$.

The 4th experiment was made differently. The horse was bled simultaneously from the right carotid and the left jugular, and the blood made to flow into two separate porous cells. On immersing into them two platinum electrodes, very powerful reaction instantly followed, and the needle after a time came to a standstill opposite 75° , keeping that position for 10'. When coagulation—not decomposition—of the blood had taken place, the needle still pointed to 70° . The direction of the current was the same as in the previous experiments, namely, from the black to the red blood.

From these researches, the author draws the following conclusions:—

Since it has been demonstrated that red and black blood when in contact, through the coats of vessels, which act like porous cells, give rise to electric reactions shown by the galvanometer, it must

be admitted that all parts of our body being traversed by the blood, there must of necessity be a constant discharge of electricity even in the most delicate tissues. 2ndly. That every organic molecule is unceasingly stimulated by the escaping electric fluid, and that it is principally under the influence of this constant excitation that all our functions are performed. Thus the oxygen contained in red blood burns the organic molecules with which it comes in contact, and produces heat. Again, during digestion, it is under the influence of electricity that the process of election of nutritive molecules, and afterwards assimilation, is carried on. The same obtains with respiration, and with internal and external secretions; in a word, with all our functions, however simple or complicated they may be. Electricity is the prime mover of all organic processes, and when it ceases all comes to a stop. Furthermore, this disengaged electricity is instantly recomposed, and there is no escape of free electricity from the body.

The phenomena are in perfect accordance with the electric phenomena developed during combustion; for it is well known that during the latter process charcoal becomes charged with negative electricity, and the surrounding atmosphere with positive electricity, or, more exactly, a current passes from the charcoal to the oxygen of the air.* Now, the principal effect of red blood is to produce actual combustion in our tissues, on account of the oxygen it contains.

*On Electricity as the Principle which causes the Vitality
and Coagulating Property of the Blood.*

By Dr. R. C. SHETTLE.

(*Lancet*, August 27 and September 12, 1863.)

The electricity which Dr. Shettle believes to be present in arterial blood is regarded as the cause of the transformation of arterial into venous blood. Of the experiments which show the presence of electricity in arterial blood and its absence in venous blood, this is one:—

“ *Experiment.*—The jugular vein of a dog (under the influence of chloroform) having been secured by means of two ligatures, was divided, and a glass tube inserted into the upper portion. Blood was then drawn into a clean glass vessel, taking care to avoid exposure to air as much as possible. A cork, in which was placed two platinum electrodes, covered with folds of blotting-paper and bladder, and upon which electrodes some pains had been bestowed to render them homogeneous, was then accurately fitted into the mouth of the vessel, and the wires of the electrodes applied at once to the galvanometer. The same alteration of the needle immediately followed that had succeeded the immersion of the plates into water—viz., a diver-

* Gaugain, On the Development of Electricity during Combustion—*Comptes Rendus de l'Acad. des Sciences*, tom. xxxviii. p. 731. Paris, 1854.

gence of 45° to the negative, the needle falling back again directly to zero, where, after a slight vibration or two, it remained, showing, as far as the galvanometer was capable, the absence of electricity in the fluid vessel.

"The carotid artery of the same dog was then tied, and treated in the same way as the vein, blood being drawn into a glass vessel similar to that used for the venous blood. Electrodes in every respect precisely similar, and which also had been tested immediately before using, were inserted as before, and the wires again attached to the galvanometer. Thirty degrees of positive electricity were at once shown, and the needle was two hours and fifteen minutes returning to zero, the backward motion being very regular. The electrodes were allowed to remain in the arterial blood six hours before being again examined, when a perfectly distinct line marked the depth to which the electrodes had been immersed, the upper portion being of a very dark venous, and the lower portion remaining of a bright arterial hue. The clot was then removed, and found to be almost black, and not firmly coagulated.

"I believe this experiment is sufficient to show that the abstraction of electricity from the blood causes it to lose its arterial character, certainly with regard to colour, and, if so, must it not also be attended with loss of its stimulating property?"

Speaking of the influence of electricity in determining the coagulation of the blood, Dr. Shettle says:—

"In a previous experiment, blood was drawn from the vein and artery of a dog, likewise under the influence of chloroform, into clean vessels. Galvanic circuits, consisting of clean plates of copper and zinc, were introduced through corks which were accurately adjusted to the mouths of the vessels so as to exclude air. Coagulation in each instance proceeded *round the plates, especially the zincode*. On the second day a peculiar formation appeared proceeding from the zincode to the side of the vessel, which might be compared not inaptly to the growth of a fibrous clot. This formation in the venous blood was loose, and the cellular spaces between the fibres large; but in the arterial, the growth was firm in texture and smaller in size as compared with the venous. This process was allowed to go on four days, when it had apparently ceased to increase. The vessels were then carefully opened; but, owing to the plates being attached to the corks, this could not be done without disturbing the formation above alluded to, and which at once became broken up, and sank to the bottom of the vessel, presenting under the microscope granular cells. The clot in both instances occupied not only the space between the plates, but also encrusted the zinc plate with a very firm coagulum; while on the side of the copper plate most distant from the zinc, it was scarcely to be called a fibrous clot, having more the appearance of transparent jelly. The clot taken from the zinc pole in the arterial blood had become as hard as a piece of dried leather, and, after exposure to the air, could be broken. That the coagulation of the blood is due to the action of a current of electricity, cannot, I think, be reasonably doubted, inasmuch as I have shown that electricity does exist in arterial blood;

and although I have not been able to detect it in venous blood, it is possible that it may exist in very small proportion, and so account for the very feeble coagulation of venous blood when perfectly excluded from the air."

On the Identity of Hæmatoidin and Bilifulvin.

By Dr. MAX JAFFE, of Berlin.

(*Virchow's Archiv*, Bd. xxiii., 1861; and *Medico-Chirurgical Review*, October, 1862.)

Dr. Jaffe, after alluding to the researches of Zencker, Brücke, Valentine, Kühne, and others, on the same subject, states that he has obtained crystals in a chloroform solution of bile, which in form and all known reactions closely agree with hæmatoidin. He then proceeds to describe an examination of a cerebral apoplectic cicatrix, which proved that hæmatoidin and bilifulvin were identical.

This cicatrix, which was of a yellowish-brown colour, and showed under the microscope a large number of crystals of hæmatoidin, was dried in a water-bath and cut into small pieces. A chloroform extract was made, and was then moistened with a few drops of absolute alcohol, by which the action of the chloroform was apparently facilitated. The extract which contained the cerebral fat was of a deep yellow colour. The chloroform extract was then gently evaporated down in a watch-glass, and for twenty-four hours was placed in a darkened place (for fear of oxidation changes, which, in the case of bilifulvin solutions, take place in the sunlight, and quickly produce changes of colour), and then examined microscopically. It was found to consist of transparent, golden-yellow, beautifully formed crystals, corresponding accurately to hæmatoidin in form.

On freeing the crystals by ether from fat, a portion of them became dissolved therein (pure ether as well as pure alcohol partially dissolves bilifulvin), the remaining crystals being soluble with tolerable facility in solution of carbonate of soda. The yellow solution became green during filtration. A small remaining portion was treated with sulphuric acid and examined, and the crystals then showed the well-known colour-play of biliary colouring-matter. Similar changes were observed in the chloroform solution on the addition of sulphuric acid.

From these observations on the microscopical and chemical character of the crystals, the author concludes that hæmatoidin and bilifulvin are identical, and he points out the bearing which this statement has upon our views regarding the changes of blood-cells in the liver.

Examinations of other apoplectic changes in portions of brain which had been long immersed in spirit did not disclose crystals of bilifulvin.

The fat which remained after distillation of the yellow chloroform extract assumed the shape of margaric acid crystals, and probably mechanically hindered the formation of bilifulvin crystals.

On the Existence of a System of Anastomosing Arteries between and connecting the Visceral and Parietal Branches of the Abdominal Aorta.

By WILLIAM TURNER, Senior Demonstrator of Anatomy,
University of Edinburgh.

(*Medico-Chirurgical Review*, July, 1863.)

From a series of careful injections, the general results of which are recorded in this paper, Dr. Turner concludes:—

“ 1st. That in the sub-peritoneal fat and areolar tissue a system of anastomosing arteries exists of greater extent and importance than has been hitherto generally recognised.

“ 2nd. That there is a much greater amount of communication between the different branches of the abdominal aorta than is commonly supposed.

“ 3rd. That not only is it possible to inject the arteries of the abdominal wall from those of the viscera, but that to some extent the bloodvessels of one viscous may be injected from those of another, and this not through the main trunk from which they both proceed, but through their mutual communications with an intermediate set of anastomosing arteries.

“ 4th. Into the question of the efficacy or inefficacy of local blood-lettings in inflammations of the abdominal viscera, it is not my intention to enter; but this may with certainty be said, that so much of the argument against its efficacy, as is based upon a supposed want of communication between the bloodvessels of the viscera and those of the wall, rests upon an assumption, and is not supported by careful experiment and observation.”

On Nervous Action, &c.

By DR. RADCLIFFE.

(*Medical Times and Gazette*, Sept. 3, 1864.)

The following remarks, which form the substance of a letter in answer to a blundering review, contain in a few words the main drift of Dr. Radcliffe's views on this subject. The writer says:—

“ In the view of nervous action proposed in these ‘Lectures’* I attach primary importance to the influence of the natural electricity which is inherent in living nerve; and herein is my great difficulty, for as yet the great facts upon which I in reality base my argument are not fully recognised and appreciated as facts—at least, in this country. I argue in this manner.

“ It is a fact that living nerve is charged with electricity during

* Lectures on Epilepsy, Pain, Paralysis, and certain other Affections of the Nervous System, delivered at the Royal College of Physicians of London, Post Svo. Churchill, 1864.

the state of rest, the longitudinal surface being electrified positively, and the transverse section negatively. Of the presence of this electricity there can be no doubt; of its condition it is not possible to speak so positively, but the evidence, so far as I can judge, shows very clearly that this condition is one of static and not of current electricity—that, in fact, the ‘nerve-current’ of Professor Du Bois-Reymond is a secondary, and not a primary phenomenon.

“It is a fact that the electricity which is inherent in the nerve during the state of rest is discharged when the nerve passes from the state of rest into that of action, and that this discharge produces a decided commotion, an electric storm, in the space beyond the nerve. This discharge, as Professor Matteucci, its discoverer, shows, is analogous to that of the torpedo: nay, it may even be identical with this latter discharge, for I am disposed to think that the nerves of the electric organ do not differ essentially from other nerves, and that the characteristic discharge of the organ is nothing more than the ordinary discharge which is developed in and around every nerve when it passes from the state of rest into that of action, with this sole difference, that it is multiplied by the cells of the organ, and then directed out of the body, instead of being expended within the body. At any rate, it is a fact that an unmistakeable electric discharge is developed in and around the nerve when the nerve passes from the state of rest into that of action.

“It is a fact that the relative electrical relations of the longitudinal surface and transverse section of living nerve may be reversed, the longitudinal surface being electrified negatively and the transverse section positively, and that this electrical reversal is brought about by the agents which produce nervous action, and at times when the vital and electrical properties of the nerve are upon the point of dying out. It is brought about at the part acted upon by pinching, pricking, or otherwise injuring the nerve; it happens in certain parts of the nervous system when the vitality and electricity of the parts have become greatly enfeebled.

“Now, these three facts supply the foundations upon which I construct my theory of nervous action.

“The first fact shows me that living nerve during the state of rest is provided with an agent whose *modus operandi* is more or less intelligible, namely, electricity.

“The second fact shows me how muscular motion or sensation may be brought about by nervous action, for it is easy to perceive that the electrical discharge, analogous to that of the torpedo, which discharge gives rise to a decided commotion, an electric storm, in the space around the nerve, may give rise to muscular contraction or sensation, if a living muscular fibre or a sensory ganglionic cell happen to be included within range of this discharge.

“The third fact shows me how a state of action may be set up in a nerve. It does so, because it is not difficult to understand that to bring parts electrified with negative electricity into relation with parts electrified with positive electricity, and, *vice versa*, to bring parts electrified with positive electricity into relation with parts electrified with negative electricity—which bringing together is the

necessary result of a partial electrical reversal of the electricities of the longitudinal surface and of the transverse section of the nerve—is to bring opposite electricities together—is to do what must issue in electrical discharge, for it is a law of electricity that similar electricities repel each other, and that opposite electricities attract each other, unite, and disappear as a discharge. This question is one which cannot be explained in a few words, and therefore, I must refer to my 'Lectures' for any further information respecting it.

" Proceeding further with the inquiry into the physiology of nervous action, I find reason to believe that the action of the blood and of 'nervous influence,' so called, is one thing in *involuntary* nervous action, and another thing in *voluntary* nervous action, but that in neither case is the conclusion really contradictory to that which has been already arrived at.

" It appears to be a fact that *involuntary* nervous action is associated with a deficient supply of arterial blood to the nervous centres. Thus, for example, the last moments of life are spent in convulsion when an animal is killed by bleeding or strangling. Involuntary nervous action in its most marked form, that is to say, is developed under circumstances in which the supply of arterial blood to the nervous centres is very deficient, and in which the development of nervous influence must be almost at zero, if, as it must needs be, this development holds any direct relation to the supply of arterial blood to these centres. Moreover, it is difficult to believe that the muscles owe their power of contracting under these circumstances to the action of the blood. For is it not a fact that the muscles which are least supplied with blood during life are most disposed to remain in the state of contraction when once in this state? And is it not also a fact that muscle passes into a state of rigor mortis when the blood is stagnant and dead, and that it relaxes and recovers its vitality and electricity when blood is again supplied to its vessels?

" It appears likewise to be a fact that *involuntary* nervous action is associated with a deficient development of 'nervous influence' in the nervous centres. Much evidence is furnished in support of this statement, but it is sufficient to refer to the great fact already cited, namely this—that convulsion is the attendant upon death by haemorrhage or suffocation; for if the development of 'nervous influence' in the nerve centres is proportionate to the amount of arterial blood supplied to the centres, it follows as a necessary consequence, that this development must be well nigh at a standstill when the nervous action which gives rise to the convulsion is at its height.

" With respect to *voluntary* muscular action, the case is very different. In this case it is plain that the proper manifestation of the will is connected with a certain degree of activity in the functions of circulation and innervation. It is plain, in fact, that voluntary muscular action would be out of the question in the case which has just been instanced as that in which involuntary nervous action is most marked—namely, in convulsion.

" How, then, is it possible to explain the fact that *involuntary*

nervous action is associated with deficient supply of arterial blood to the nervous centres, and with deficient development of nervous influence in these centres? Is it to be supposed that this state implies the production of that reversal of electricity in some part of the nervous system—perhaps in the medulla oblongata—of which I have spoken as concerned in the causation of the nervous action? Such a supposition is possible, for it has been seen that this reversal happens when the vitality of the nervous system is at a very low ebb—under circumstances which may well be supposed to exist in the last moments of life in death by haemorrhage or suffocation. And such a supposition is all that is necessary according to my view of nervous action. I therefore adopt this supposition in preference to the current opinion, which ascribes increased nervous action to increased development of ‘nervous influence’; for, with less arterial blood supplied to the nervous centres than usual, how is it possible that more ‘nervous influence’ than usual can be developed in these centres?

“Nay, I am disposed to think that in *voluntary* nervous action the will may act by producing the same reversal of the electricity of the nervous system in the part acted upon as that which is produced by any agent having the power of originating nervous action; and thus, although an active condition of the circulation and innervation is necessary to the manifestation of the *will*, it does not follow that the same active condition is necessary to the existence of the nervous action produced by the *will*.

“After what I have said of the physiology of nervous action, I may dispose, in a few words, of what I have now to say respecting the physiology of muscular action and respecting the pathological problems with which I am specially concerned—namely, convulsion, tremor, spasm, and pain of a neuralgic character.

“The simple fact is, that the electrical law of muscle during rest and action is precisely the same as the electrical law of nerve during rest and action. What I believe of rigor mortis is this—that the contraction is the result of the muscle being left to the unresisted operation of the attractive force which is inherent in the physical constitution of the muscular molecules by the dying-out of the electricity which was inherent in the muscle during life. And what I believe of ordinary muscular contraction is this—that the electric discharge, analogous to that of the torpedo, which is developed when the motor nerve passes from the state of rest into that of action, gives rise to contraction in the muscular fibre which comes within its range, not by acting as a stimulus to a vital property of irritability in the fibre, but by suspending for the moment the electricity which is inherent in the fibre during the state of rest, and so leaving the muscular molecules free to yield to the action of the attractive force which is inherent in the physical constitution of these molecules. The view here taken is one in which ordinary muscular contraction is supposed to be essentially nothing more than the momentary passing of the muscle into the state of contraction which obtains permanently in rigor mortis. The view is one in which it is also supposed that living muscle, left to itself, instantly recovers its in-

herent electricity, and remains in the state of relaxation so long as this electricity continues to be developed.

"With respect to the pathological problems with which I have to do—namely, convulsion, tremor, spasm, and pain of a neuralgic character, I will only say broadly that the facts, so far as I have been able to observe them, are precisely what they should not be, according to the current view of nervous action, and precisely what they should be according to the theory of nervous action advanced in my 'Lectures.' What I find, indeed, is simply this—that convulsion, tremor, spasm, and pain of a neuralgic character, are associated with a depressed condition of the circulation, and not with the opposite condition of active febrile or inflammatory excitement. I find a state of things which I cannot explain, if, according to the current theory, I am to suppose that convulsion, tremor, spasm, or neuralgic pain, is the sign of vital excitement in the nervous system; for in this case I require a corresponding state of increased action in the circulation to account for this excitement. I find, on the other hand, that very state of things which I require, if I am to explain convulsion, tremor, spasm, or neuralgic pain, according to my view of nervous action; for according to this view the electrical reversal which brings about the state of nervous action will not happen unless the circulation be in a very depressed state. The question is a simple one. It is—Are the facts as I represent them to be? This is a matter for simple clinical investigation, and not for profound physiological research. It is a matter, too, which may be disposed of before entering upon the consideration of the physiological portion of my argument, for in reality the physiological and pathological portions of this argument are in great measure capable of being considered independently of each other."

On the Functions of the Cerebellum.

By Dr. W. H. DICKINSON.

(*Proceedings of the Royal Society*, April 7, 1864.)

This paper is divided into two Parts; the first gives the results of experiments on animals; the second, of observations upon the human being.

PART I.

Assuming that the great divisions of the brain preserve each the same function through the vertebrate kingdom, it is maintained that experiments which can be performed only on such of the lower animals as are very tenacious of life, will afford deductions of universal application.

The method of proceeding with regard to each species was to remove, first, the whole encephalon, with the exception of the medulla oblongata; then in a similar animal only the cerebrum was taken away. The only difference between the two cases was in the fact that one animal had a cerebellum, and the other had not. A

comparison was believed to show, in the powers which one had more than the other, the function of the organ the possession of which constituted the only difference.

Finally it was ascertained in each species what is the effect of taking away the cerebellum alone.

The use of the organ was thus estimated in two ways—by the effect of its addition to the medulla, and of its subtraction from the rest of the nervous system.

The species so treated are arranged in an ascending scale, according to the comparative weight of the cerebellum. The field-snake, frog, salamander, toad, land-tortoise, eel, water-tortoise, pike, perch, tench, dace, carp, gold-fish, rudd, loach, and gudgeon were subjected to these operations; besides which, many experiments of a less systematic character were upon birds and mammalia.

The results are these:—

In *Reptiles*, with the exception of the snake, the cord, together with the medulla oblongata, is sufficient to give the power of voluntary or spontaneous motion—limited, but usually enough to allow of feeble locomotion.

With the addition of the cerebellum, all actions dependent on the will appear to be naturally performed.

The removal of the cerebellum shows that the cerebrum by itself is unable to give more than a limited amount of voluntary motion, and that of a kind deficient in balance and adjustment.

It is therefore inferred that the cord, together with the medulla oblongata, is a great source of spontaneous motor power, in which function both the cerebrum and the cerebellum take part, the cerebellum to the greater extent; it also appears that a certain harmony in the use of the muscles depends on the possession of the latter organ.

Regarding *Fishes*, the cord and medulla oblongata seem unequal to the performance of voluntary motion.

When the cerebellum is added, the powers become so far extended that movements are made in obedience to external stimuli. Generally speaking, a determined position is maintained and locomotion accomplished, without the use, however, of the pectoral fins.

If the cerebellum only be taken away, there is a loss of the proper adjustment between the right and left sides; so that oscillation or rotation takes place. All the limbs are used, but apparently with a deficiency of sustained activity.

It is therefore concluded that with *Fishes*, as with *Reptiles*, the power of intentional movement is shared by both cerebrum and cerebellum; the former in this case has the larger influence.

Such movements as depend on the cerebrum are destitute of lateral balance, are sudden in being affected by any external cause, and are emotional in their character. Such as depend on the cerebellum are mutually adjusted, of a continuous kind, and less directly under the influence of consciousness.

The same facts were supported by experiments on the higher orders of animals: in these it seemed that the cord and medulla are insufficient to excite voluntary movements. The muscles, as with

fishes and reptiles, acknowledge a double rule, from the cerebrum and from the cerebellum. The anterior limbs are most subservient to the cerebrum; the posterior to the cerebellum. The limbs on one side are in connexion chiefly with the lobe of the opposite side. The absence of the cerebellum destroys the power of lateral balance.

From the negative results of the experiments, it is inferred that the cerebellum has nothing to do with common sensation, with the sexual propensity, with the action of the involuntary muscles, with the maintenance of animal heat, or with secretion.

The only function which the experiments assigned to the cerebellum is such as concerns the voluntary muscles, which receive therefrom a regulated supply of motor influence. Each lateral half of the cerebellum affects both sides, but the one opposite to itself most.

The cerebellum has a property distinct from its true voluntary power, which harmonizes the action of the voluntary muscles, and has been described as "co-ordination."

The voluntary muscles are under a double influence—from the cerebrum and from the cerebellum. The anterior limbs are chiefly under the influence of the cerebrum; the posterior, of the cerebellum. Cerebellar movements are apt to be habitual, while cerebral are impulsive. The cerebellum acts when the cerebrum is removed, though when both organs exist it is under its control.

PART II.

From an analysis of one case of congenital absence of the cerebellum, one of disease of the whole organ, and 46 of disease of a portion of it, the following deductions are stated:—

The only faculty which constantly suffers in consequence of changes in the cerebellum, is the power of voluntary movement.

When the organ is absent or defective congenitally, we have want of action in the muscles of the lower extremities.

When the entire structure is changed by disease, we have loss of voluntary power, either general throughout the trunk, or limited to the lower limbs—which results are about equally frequent.

From the manner in which the paralysis was distributed in cases of disease of a part of the organ, it is inferred that each lobe is in connexion as a source of voluntary movement with all the four limbs, but in the greatest degree with the limbs of the opposite side, and with the lower more than with the upper extremities.

The occasional occurrence of loss of visual power, and alterations of the sexual propensity, is referred to the conveyance of irritation to the corpora quadrigemina in one case, and the spinal cord in the other.

From both sources of knowledge it is concluded that the cerebellum has distinct offices.

It is a source of voluntary motor power to the muscles supplied by the spinal nerves. It influences the lower more than the upper limbs, and produces habitual rather than impulsive movements. Each lobe affects both sides of the body, but most that opposite to itself.

Secondly, the cerebellum has a power which has been described as that of "co-ordination," which is similarly distributed.

Finally, it is suggested that the outer portion of the organ may be the source of its voluntary motor power, while its inner layer is the means of regulating its distribution.

On the Production of Heat by Nerves during their Period of Activity.

By Professor VALENTIN.

(*Virchow's Archiv*, xxviii, 1 & 2, 1864; and *Gaz. Hebdom.*, Juillet 15, 1864.)

In his thermo-electric researches on muscular contraction, Helmholtz stated that he had only obtained negative results in his experiments on nerves, and that if any heat were really produced by these organs during the period of their functional activity, it must be only to an infinitesimal amount, not greater than a few thousandths of a degree (*Müller's Archiv*, 1848). This opinion, however, could, à priori, be accepted with difficulty. Nerves, when inactive, absorb oxygen and exhale carbonic acid, just as muscles do, as well as other tissues bathed in blood; and this interchange of gases is too great to be accounted for by the small quantity of blood which remains in the nerves under examination (*Valentin, Archiv für physiol. Heilkunde*, 1859). On the other hand, numerous analogies exist between the electro-motor properties of nerves and muscles, both during the period of rest, and that of activity.

Two circumstances, however, could apparently oppose the inference that nerves, like muscles, must produce heat. First, a muscle, when thrown into contraction, is altered in shape, whilst its volume remains the same, or scarcely undergoes any appreciable change. Secondly, there is a relative increase in the quantity of carbonic acid exhaled, and a diminution in the amount of oxygen absorbed (*Valentin, Szczeklow*).

Experiments alone could therefore solve the problem, and these Valentin carried on with an extremely delicate magnetic needle and a thermo-electric apparatus capable of being influenced by $\frac{1}{222}$ part of a degree Centigrade.

The nerves of the sciatic plexus of a frog (*Rana temporaria*), were excited by galvanizing the spinal cord for the space of half a minute. The production of heat became manifest immediately on the galvanism being used, but on ceasing it the needle oscillated for some time, and then stood at rest. The experiment could be repeated three or four times with always the same results, the only difference being that the production of heat was greater in the first two trials. The following table shows the progressive production of heat in a series of six experiments made with a large frog (*Rana esculenta*). The third column gives the degree at the moment of interrupting the galvanic excitation; the fifth and sixth, the amount of heat observed a few minutes after the cessation of the current. A comparison of the numbers in each column shows that the greatest

amount of heat is produced shortly after ceasing to galvanize the cord.

Galvanization.	Number of seconds.	Temperature at the end of the excitation. C.	Difference from previous observation.	Number of minutes after the excitation.	Temperature C.	Difference from previous observation.
I.	90	0,021	+0,021	1	0,025	+0,004
				6	0,054	+0,029
II.	30	0,064	+0,010	4	0,068	+0,004
				6	0,071	+0,003
				8	0,080	+0,009
				12	0,084	+0,004
III.	30	0,087	+0,003	2	0,088	+0,001
				3	0,089	+0,001
				5½	0,090	+0,001
IV.	30	0,092	+0,002	1	0,092	0,000
V.	30	0,094	+0,002	1	0,094	0,000
				5	0,096	+0,002
				7	0,098	0,002
VI.	30	0,099	+0,001	"	"	"

The maximum production of heat was obtained on first galvanizing the cord for the space of ninety seconds. The next applications of galvanism became less and less efficacious, until the increase of temperature was no more than the thousandth part of a degree Centigrade. This degree in the series peremptorily shows that the increase first observed could not be due to some modification of the apparatus itself.

Mechanical irritation produced exactly similar results in other frogs, and the author accordingly draws the following conclusions: That irritation of the spinal cord produces an appreciable increase of temperature in the sciatic plexus, and that nerves produce heat during the period of their functional activity.

On the Termination of Nerves in Muscles, as observed in the Frog, and on the Disposition of the Nerves in the Heart of the Frog.

By Professor A. KÖLLIKER.

(*Proceedings of Royal Society, May 1, 1864.*)

The more interesting observations described in this essay are—

1. As the nerves are confined in their

inferences from the facts described (p. 32) are these—
ia, and are con-
portion of the

muscular fibre, it may be inferred that there must be action at a distance.

2. As the ends of the motor nerves are pale fibres destitute of medullary sheath, it would appear that the latter is of but secondary importance. The same fact may, perhaps, also afford an explanation of the special action of certain poisonous agents, as the urari, on the ends of these nerves.

3. The muscles have numerous sentient nerve-fibres distributed on their surface, or on the surface of their larger divisions.

4. The heart, at least the heart of the frog, has two distinct sets of nervous fibres, those of the pneumogastric and those from the ganglion-cells, which are both distributed to the muscles. The vagus therefore, acts directly on the muscular fibres of the organ, and the well-known experiment of Weber can scarcely be explained through a supposed action of the vagus on the ganglia. On the other hand, the ganglia and their fibres are also motorial organs of the heart, and alone act when it is separated from the body.

*On the Distribution of Nerves to the Elementary Fibre
of Striped Muscle.*

By Dr. BEALE, Professor of Physiology in King's College,
London.

(*Proceedings of Royal Society, June 19, 1862.*)

The conclusions arrived at in this paper are these :—

1. In certain muscles of the frog the distribution of dark-bordered nerve-fibres is pretty uniform in every part. Although in the case of the pectoral a greater number of nerve-fibres is distributed to the central part of the muscle, fibres may be traced from the large bundle almost to the extremities of some of the muscular fibres. Many branches which easily escape observation pass between the muscular fibres, and their subdivisions supply neighbouring fibres, or are gradually lost in the connective tissue.

2. Fine nerve-fibres are most easily demonstrated on the external surface of the sarcolemma near the nerve-trunks ; but reasons have been advanced in favour of the conclusion that every elementary muscular fibre is more or less freely supplied with nerve-fibres throughout its entire length. Many of the fine nerve-fibres on the surface of the muscular fibres become gradually very faint, until from their extreme tenuity we are no longer able to follow them.

3. Fine nerve-fibres in direct continuation with the dark-bordered fibres, and less than the $\frac{1}{50}$. $\frac{1}{50}$ th of an inch in diameter, have been seen to divide into finer branches which have nuclei in connexion with them.

4. The pale fibres delineated by Kühne and Kölliker, and by them considered terminal, consist of—

a. Fibres about the $\frac{1}{50}$. $\frac{1}{50}$ th of an inch in diameter, or less, resulting from the subdivision of the dark-bordered fibre.

b. Fibres resulting from the subdivision of fine nerve-fibres

ramifying in the sheath of the dark-bordered fibre, or situated external to it.

5. Nuclei are found in connexion with—

a. The dark-bordered fibre itself, near its terminal ramifications.

b. The fine fibres which are the direct continuation of the dark-bordered fibres.

c. The fine fibres in the sheath, or external to it.

6. The nuclei and delicate fibres above referred to are arranged so as to form networks, the meshes of which vary much in size, situated with the capillaries on the external surface of the sarcolemma. The fibres of this network are compound, and consist of finer fibres which are distinct from, and do not anastomose with, each other. The fine fibres continued from some of the dark-bordered fibres, as well as those ramifying in the sheath of the nerves, may sometimes be followed over six or more elementary muscular fibres, and form, with other fine branches, networks, many of the meshes being as wide as a muscular fibre.

7. Fine nerve-fibres with nuclei connected with them exist (not unfrequently to the number of four or five) in the sheath of the dark-bordered nerve-fibres near their distribution; and some are also found external to what appears to be the outline of the sheath. Some of these result from the subdivision of a dark-bordered fibre.

These fine fibres and their nuclei have been hitherto included under the head of "connective tissue."

8. The connective tissue around the elementary muscular fibres, and in connexion with the nerve-fibres, is composed of—

a. Nuclei which might have taken part in the formation of the nerve-fibres, but which have degenerated, and a low form of fibrous tissue has alone been produced.

b. Fibres and nuclei which were once active, and formed an integral part of the nervous system, but which have grown old, and have been replaced by new nuclei and fibres.

c. The remains of altered and wasted vessels and nerve-fibres distributed to them, and wasted muscular fibres themselves.

9. The nerves distributed to the voluntary muscles of the frog do not terminate in free ends, but there is reason for believing that complete nervous circuits exist. In all cases the fibres resulting from the division of the ordinary nerve-fibres are so fine that many cannot be seen with a power magnifying less than 1000 diameters, and there is evidence of the existence of fibres which could only be demonstrated by employing a much higher magnifying power. It is by these very fine fibres alone, and their nuclei, that the tissues are influenced. The ordinary nerve-fibres are only the cords which connect this extensive peripheral system, which has been traced in different tissues far beyond the point to which the dark-bordered nerve-fibres can be followed, with the central organs of the nervous system.

10. The facts and conclusions above stated, with reference to the distribution of nerve-fibres to the voluntary muscles of the frog, are in accordance with the arrangement of the finest nerve-fibres de-

monstrated in many other tissues of the same animal, and agree with many appearances observed by the author in connexion with the peripheral distribution of the nerves, not only in certain tissues of man and the higher mammalia, but also in invertebrate animals.

11. The distribution of the finest branches of the nerve-fibres can only be demonstrated in tissues which have been immersed in fluids which refract highly, as syrup or glycerine.

Experiments on the Re-union of Sensitive and Motor Nerves.

By MM. GLUGE and THIERNESSE.

(*Bull. de l'Acad. Royale de Belgique*, t. xvi., 1863; and *Gaz. Hebdom. de Méd. et de Chir.*, No. 26, June 17, 1864.)

The authors proposed to themselves to determine whether the functions of nerves are inherent in the fibres themselves, or whether they solely depend on their central origin and the tissues to which they are distributed. From previous experiments, they had already drawn the conclusion that it is impossible to convert a sensitive into a motor fibre, or vice versa, for a motor fibre can only conduct impressions in a centrifugal, and a sensitive in a centripetal direction.

Philippeaux and Vulpian, however, subsequently drew from their experiments entirely opposite conclusions. They connected the central end of the lingual nerve with the peripheral extremity of the hypoglossal, and after having divided the lingual a few months afterwards above the point of union, they obtained contractions of the corresponding half of the tongue, on mechanically irritating the peripheral end of the lingual, then united with the hypoglossal, the fibres of a sensitive nerve conducting a motor impression.

In consequence of these published results, Gluge and Thiernesse determined to repeat the experiments. They were performed on two dogs on January 29th and February 6th, 1863; they were borne well, and the only trace left behind was a deviation of the tongue towards the side of the operation when protruded out of the mouth. The dogs were killed on June 3rd by division of the spinal marrow. In the first dog, the two nerves were completely united: after division of the lingual above the scar, the peripheral extremity in connexion with the hypoglossal was pinched, but no contraction could be detected of the muscles of the tongue, whereas the passage of an electric current immediately produced contraction. On the other side mechanical irritation of the hypoglossal instantly excited muscular contraction.

In the second dog there was equally perfect union of the two nerves. On pinching the lingual three minutes after the animal's death violent contractions of the muscles of the tongue were observed; but after the lingual had been divided above the cicatrix, its peripheral end was ineffectually irritated; no contraction followed, whilst compression of the scar itself—that is of the peripheral extremity of the hypoglossal—caused powerful muscular contraction. It is manifest therefore that the first movements noted were due to a reflex

necessary result of a partial electrical reversal of the electricities of the longitudinal surface and of the transverse section of the nerve—is to bring opposite electricities together—is to do what must issue in electrical discharge, for it is a law of electricity that similar electricities repel each other, and that opposite electricities attract each other, unite, and disappear as a discharge. This question is one which cannot be explained in a few words, and therefore, I must refer to my 'Lectures' for any further information respecting it.

" Proceeding further with the inquiry into the physiology of nervous action, I find reason to believe that the action of the blood and of 'nervous influence,' so called, is one thing in *involuntary* nervous action, and another thing in *voluntary* nervous action, but that in neither case is the conclusion really contradictory to that which has been already arrived at.

" It appears to be a fact that *involuntary* nervous action is associated with a deficient supply of arterial blood to the nervous centres. Thus, for example, the last moments of life are spent in convulsion when an animal is killed by bleeding or strangling. Involuntary nervous action in its most marked form, that is to say, is developed under circumstances in which the supply of arterial blood to the nervous centres is very deficient, and in which the development of nervous influence must be almost at zero, if, as it must needs be, this development holds any direct relation to the supply of arterial blood to these centres. Moreover, it is difficult to believe that the muscles owe their power of contracting under these circumstances to the action of the blood. For is it not a fact that the muscles which are least supplied with blood during life are most disposed to remain in the state of contraction when once in this state? And is it not also a fact that muscle passes into a state of rigor mortis when the blood is stagnant and dead, and that it relaxes and recovers its vitality and electricity when blood is again supplied to its vessels?

" It appears likewise to be a fact that *involuntary* nervous action is associated with a deficient development of 'nervous influence' in the nervous centres. Much evidence is furnished in support of this statement, but it is sufficient to refer to the great fact already cited, namely this—that convulsion is the attendant upon death by haemorrhage or suffocation; for if the development of 'nervous influence' in the nerve centres is proportionate to the amount of arterial blood supplied to the centres, it follows as a necessary consequence, that this development must be well nigh at a standstill when the nervous action which gives rise to the convulsion is at its height.

" With respect to *voluntary* muscular action, the case is very different. In this case it is plain that the proper manifestation of the will is connected with a certain degree of activity in the functions of circulation and innervation. It is plain, in fact, that voluntary muscular action would be out of the question in the case which has just been instanced as that in which involuntary nervous action is most marked—namely, in convulsion.

" How, then, is it possible to explain the fact that *involuntary*

nervous action is associated with deficient supply of arterial blood to the nervous centres, and with deficient development of nervous influence in these centres? Is it to be supposed that this state implies the production of that reversal of electricity in some part of the nervous system—perhaps in the medulla oblongata—of which I have spoken as concerned in the causation of the nervous action? Such a supposition is possible, for it has been seen that this reversal happens when the vitality of the nervous system is at a very low ebb—under circumstances which may well be supposed to exist in the last moments of life in death by haemorrhage or suffocation. And such a supposition is all that is necessary according to my view of nervous action. I therefore adopt this supposition in preference to the current opinion, which ascribes increased nervous action to increased development of ‘nervous influence’; for, with less arterial blood supplied to the nervous centres than usual, how is it possible that more ‘nervous influence’ than usual can be developed in these centres?

“Nay, I am disposed to think that in *voluntary* nervous action the will may act by producing the same reversal of the electricity of the nervous system in the part acted upon as that which is produced by any agent having the power of originating nervous action; and thus, although an active condition of the circulation and innervation is necessary to the manifestation of the *will*, it does not follow that the same active condition is necessary to the existence of the nervous action produced by the *will*.

“After what I have said of the physiology of nervous action, I may dispose, in a few words, of what I have now to say respecting the physiology of muscular action and respecting the pathological problems with which I am specially concerned—namely, convulsion, tremor, spasm, and pain of a neuralgic character.

“The simple fact is, that the electrical law of muscle during rest and action is precisely the same as the electrical law of nerve during rest and action. What I believe of rigor mortis is this—that the contraction is the result of the muscle being left to the unresisted operation of the attractive force which is inherent in the physical constitution of the muscular molecules by the dying-out of the electricity which was inherent in the muscle during life. And what I believe of ordinary muscular contraction is this—that the electric discharge, analogous to that of the torpedo, which is developed when the motor nerve passes from the state of rest into that of action, gives rise to contraction in the muscular fibre which comes within its range, not by acting as a stimulus to a vital property of irritability in the fibre, but by suspending for the moment the electricity which is inherent in the fibre during the state of rest, and so leaving the muscular molecules free to yield to the action of the attractive force which is inherent in the physical constitution of these molecules. The view here taken is one in which ordinary muscular contraction is supposed to be essentially nothing more than the momentary passing of the muscle into the state of contraction which obtains permanently in rigor mortis. The view is one in which it is also supposed that living muscle, left to itself, instantly recovers its in-

necessary result of a partial electrical reversal of the electricities of the longitudinal surface and of the transverse section of the nerve—is to bring opposite electricities together—is to do what must issue in electrical discharge, for it is a law of electricity that similar electricities repel each other, and that opposite electricities attract each other, unite, and disappear as a discharge. This question is one which cannot be explained in a few words, and therefore, I must refer to my 'Lectures' for any further information respecting it.

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nervous action is associated with deficient supply of arterial blood to the nervous centres, and with deficient development of nervous influence in these centres? Is it to be supposed that this state implies the production of that reversal of electricity in some part of the nervous system—perhaps in the medulla oblongata—of which I have spoken as concerned in the causation of the nervous action? Such a supposition is possible, for it has been seen that this reversal happens when the vitality of the nervous system is at a very low ebb—under circumstances which may well be supposed to exist in the last moments of life in death by haemorrhage or suffocation. And such a supposition is all that is necessary according to my view of nervous action. I therefore adopt this supposition in preference to the current opinion, which ascribes increased nervous action to increased development of ‘nervous influence’; for, with less arterial blood supplied to the nervous centres than usual, how is it possible that more ‘nervous influence’ than usual can be developed in these centres?

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“ The simple fact is, that the electrical law of muscle during rest and action is precisely the same as the electrical law of nerve during rest and action. What I believe of rigor mortis is this—that the contraction is the result of the muscle being left to the unresisted operation of the attractive force which is inherent in the physical constitution of the muscular molecules by the dying-out of the electricity which was inherent in the muscle during life. And what I believe of ordinary muscular contraction is this—that the electric discharge, analogous to that of the torpedo, which is developed when the motor nerve passes from the state of rest into that of action, gives rise to contraction in the muscular fibre which comes within its range, not by acting as a stimulus to a vital property of irritability in the fibre, but by suspending for the moment the electricity which is inherent in the fibre during the state of rest, and so leaving the muscular molecules free to yield to the action of the attractive force which is inherent in the physical constitution of these molecules. The view here taken is one in which ordinary muscular contraction is supposed to be essentially nothing more than the momentary passing of the muscle into the state of contraction which obtains permanently in rigor mortis. The view is one in which it is also supposed that living muscle, left to itself, instantly recovers its in-

herent electricity, and remains in the state of relaxation so long as this electricity continues to be developed.

"With respect to the pathological problems with which I have to do—namely, convulsion, tremor, spasm, and pain of a neuralgic character, I will only say broadly that the facts, so far as I have been able to observe them, are precisely what they should not be, according to the current view of nervous action, and precisely what they should be according to the theory of nervous action advanced in my 'Lectures.' What I find, indeed, is simply this—that convulsion, tremor, spasm, and pain of a neuralgic character, are associated with a depressed condition of the circulation, and not with the opposite condition of active febrile or inflammatory excitement. I find a state of things which I cannot explain, if, according to the current theory, I am to suppose that convulsion, tremor, spasm, or neuralgic pain, is the sign of vital excitement in the nervous system; for in this case I require a corresponding state of increased action in the circulation to account for this excitement. I find, on the other hand, that very state of things which I require, if I am to explain convulsion, tremor, spasm, or neuralgic pain, according to my view of nervous action; for according to this view the electrical reversal which brings about the state of nervous action will not happen unless the circulation be in a very depressed state. The question is a simple one. It is—Are the facts as I represent them to be? This is a matter for simple clinical investigation, and not for profound physiological research. It is a matter, too, which may be disposed of before entering upon the consideration of the physiological portion of my argument, for in reality the physiological and pathological portions of this argument are in great measure capable of being considered independently of each other."

On the Functions of the Cerebellum.

By Dr. W. H. DICKINSON.

(*Proceedings of the Royal Society, April 7, 1864.*)

This paper is divided into two Parts; the first gives the results of experiments on animals; the second, of observations upon the human being.

PART I.

Assuming that the great divisions of the brain preserve each the same function through the vertebrate kingdom, it is maintained that experiments which can be performed only on such of the lower animals as are very tenacious of life, will afford deductions of universal application.

The method of proceeding with regard to each species was to remove, first, the whole encephalon, with the exception of the medulla oblongata; then in a similar animal only the cerebrum was taken away. The only difference between the two cases was in the fact that one animal had a cerebellum, and the other had not. A

comparison was believed to show, in the powers which one had more than the other, the function of the organ the possession of which constituted the only difference.

Finally it was ascertained in each species what is the effect of taking away the cerebellum alone.

The use of the organ was thus estimated in two ways—by the effect of its addition to the medulla, and of its subtraction from the rest of the nervous system.

The species so treated are arranged in an ascending scale, according to the comparative weight of the cerebellum. The field-snake, frog, salamander, toad, land-tortoise, eel, water-tortoise, pike, perch, tench, dace, carp, gold-fish, rudd, loach, and gudgeon were subjected to these operations; besides which, many experiments of a less systematic character were upon birds and mammalia.

The results are these:—

In Reptiles, with the exception of the snake, the cord, together with the medulla oblongata, is sufficient to give the power of voluntary or spontaneous motion—limited, but usually enough to allow of feeble locomotion.

With the addition of the cerebellum, all actions dependent on the will appear to be naturally performed.

The removal of the cerebellum shows that the cerebrum by itself is unable to give more than a limited amount of voluntary motion, and that of a kind deficient in balance and adjustment.

It is therefore inferred that the cord, together with the medulla oblongata, is a great source of spontaneous motor power, in which function both the cerebrum and the cerebellum take part, the cerebellum to the greater extent; it also appears that a certain harmony in the use of the muscles depends on the possession of the latter organ.

Regarding *Fishes*, the cord and medulla oblongata seem unequal to the performance of voluntary motion.

When the cerebellum is added, the powers become so far extended that movements are made in obedience to external stimuli. Generally speaking, a determined position is maintained and locomotion accomplished, without the use, however, of the pectoral fins.

If the cerebellum only be taken away, there is a loss of the proper adjustment between the right and left sides; so that oscillation or rotation takes place. All the limbs are used, but apparently with a deficiency of sustained activity.

It is therefore concluded that with *Fishes*, as with *Reptiles*, the power of intentional movement is shared by both cerebrum and cerebellum; the former in this case has the larger influence.

Such movements as depend on the cerebrum are destitute of lateral balance, are sudden in being affected by any external cause, and are emotional in their character. Such as depend on the cerebellum are mutually adjusted, of a continuous kind, and less directly under the influence of consciousness.

The same facts were supported by experiments on the higher orders of animals: in these it seemed that the cord and medulla are insufficient to excite voluntary movements. The muscles, as with

fishes and reptiles, acknowledge a double rule, from the cerebrum and from the cerebellum. The anterior limbs are most subservient to the cerebrum; the posterior to the cerebellum. The limbs on one side are in connexion chiefly with the lobe of the opposite side. The absence of the cerebellum destroys the power of lateral balance.

From the negative results of the experiments, it is inferred that the cerebellum has nothing to do with common sensation, with the sexual propensity, with the action of the involuntary muscles, with the maintenance of animal heat, or with secretion.

The only function which the experiments assigned to the cerebellum is such as concerns the voluntary muscles, which receive therefrom a regulated supply of motor influence. Each lateral half of the cerebellum affects both sides, but the one opposite to itself most.

The cerebellum has a property distinct from its true voluntary power, which harmonizes the action of the voluntary muscles, and has been described as "co-ordination."

The voluntary muscles are under a double influence—from the cerebrum and from the cerebellum. The anterior limbs are chiefly under the influence of the cerebrum; the posterior, of the cerebellum. Cerebellar movements are apt to be habitual, while cerebral are impulsive. The cerebellum acts when the cerebrum is removed, though when both organs exist it is under its control.

PART II.

From an analysis of one case of congenital absence of the cerebellum, one of disease of the whole organ, and 46 of disease of a portion of it, the following deductions are stated:—

The only faculty which constantly suffers in consequence of changes in the cerebellum, is the power of voluntary movement.

When the organ is absent or defective congenitally, we have want of action in the muscles of the lower extremities.

When the entire structure is changed by disease, we have loss of voluntary power, either general throughout the trunk, or limited to the lower limbs—which results are about equally frequent.

From the manner in which the paralysis was distributed in cases of disease of a part of the organ, it is inferred that each lobe is in connexion as a source of voluntary movement with all the four limbs, but in the greatest degree with the limbs of the opposite side, and with the lower more than with the upper extremities.

The occasional occurrence of loss of visual power, and alterations of the sexual propensity, is referred to the conveyance of irritation to the corpora quadrigemina in one case, and the spinal cord in the other.

From both sources of knowledge it is concluded that the cerebellum has distinct offices.

It is a source of voluntary motor power to the muscles supplied by the spinal nerves. It influences the lower more than the upper limbs, and produces habitual rather than impulsive movements. Each lobe affects both sides of the body, but most that opposite to itself.

Secondly, the cerebellum has a power which has been described as that of "co-ordination," which is similarly distributed.

Finally, it is suggested that the outer portion of the organ may be the source of its voluntary motor power, while its inner layer is the means of regulating its distribution.

On the Production of Heat by Nerves during their Period of Activity.

By Professor VALENTIN.

(*Virchow's Archiv*, xxviii. 1 & 2, 1864; and *Gaz. Hebdom.*, Juillet 15, 1864.)

In his thermo-electric researches on muscular contraction, Helmholtz stated that he had only obtained negative results in his experiments on nerves, and that if any heat were really produced by these organs during the period of their functional activity, it must be only to an infinitesimal amount, not greater than a few thousandths of a degree (*Müller's Archiv*, 1848). This opinion, however, could, *a priori*, be accepted with difficulty. Nerves, when inactive, absorb oxygen and exhale carbonic acid, just as muscles do, as well as other tissues bathed in blood; and this interchange of gases is too great to be accounted for by the small quantity of blood which remains in the nerves under examination (*Valentin, Archiv für physiol. Heilkunde*, 1859). On the other hand, numerous analogies exist between the electro-motor properties of nerves and muscles, both during the period of rest, and that of activity.

Two circumstances, however, could apparently oppose the inference that nerves, like muscles, must produce heat. First, a muscle, when thrown into contraction, is altered in shape, whilst its volume remains the same, or scarcely undergoes any appreciable change. Secondly, there is a relative increase in the quantity of carbonic acid exhaled, and a diminution in the amount of oxygen absorbed (*Valentin, Sczelkow*).

Experiments alone could therefore solve the problem, and these Valentin carried on with an extremely delicate magnetic needle and a thermo-electric apparatus capable of being influenced by $\frac{1}{222}$ part of a degree Centigrade.

The nerves of the sciatic plexus of a frog (*Rana temporaria*), were excited by galvanizing the spinal cord for the space of half a minute. The production of heat became manifest immediately on the galvanism being used, but on ceasing it the needle oscillated for some time, and then stood at rest. The experiment could be repeated three or four times with always the same results, the only difference being that the production of heat was greater in the first two trials. The following table shows the progressive production of heat in a series of six experiments made with a large frog (*Rana esculenta*). The third column gives the degree at the moment of interrupting the galvanic excitation; the fifth and sixth, the amount of heat observed a few minutes after the cessation of the current. A comparison of the numbers in each column shows that the greatest

The author next considers the influence of reflex action on vaso-motor nerves. There are, he says, two ways in which these nerves may act on blood-vessels :—they may either *constrict* them through existing contraction of their circular muscles, or they may *dilate* them. Active dilatation has been denied, and has been looked upon as the result of a paralysis of the constrictor muscles of the vessels, or of exhaustion of these muscles, due to a constriction which always precedes dilatation. But M. Schiff has performed experiments which prove—1st. That the vascular dilatation which follows as an effect of irritation is not always preceded by contraction of the vessel, and that there are therefore vascular dilatations which are not due to exhaustion of the circular fibres. 2nd. That the dilatation is not a mechanical result of an augmented flow of blood ; for if there were such a flow of blood, the dilatation would not be limited to the point irritated, but would be still more marked in the more central portion of the artery situated below the irritated spot. 3rd. That the dilatation is not a mechanical effect resulting from a reflex contraction situated either further on, near the periphery or in the venous system ; for such a contraction should exist either immediately above the point irritated in the continuity of the vessel (in which case it would be seen), or it would be seated in a more distant spot (in which case it should cause a dilatation of the vessel in its whole length, and not in one limited part alone). 4th. That the sympathetic contains no vascular nerves which preside over this kind of dilatation.

Dilatation produced by tickling does not take place independently of the nerves, for no such result follows after all the sensitive nerves of the part have been divided. The author regards it as a local effect of a local irritation, occurring independently of a general disturbance of the circulation. It is not passive, and must therefore be an active phenomenon. It is under the influence of the nervous system, and yet the presence of nervous trunks is not an indispensable condition of its production. In this respect it is analogous to muscular contraction, and it further resembles it in that the irritation must be increased after the nervous trunks have been cut off.

The following experiment now proves that active vascular dilatation is also dependent on a motor-nervous system which directly provokes it. The cervical portion of the left sympathetic is excised in a dog ; for many successive days or weeks after this, the left side of the head and the left ear are found warmer than the right, where the vessels are a little more dilated. If now the animal be taken out in the fields, he gets warmer all over ; both ears participate in this increase of temperature, their blood-vessels dilate a little without any previous stage of contraction. At last when the animal pants for breath, it is noticed that the blood-vessels of the *right* ear are more dilated than those of the *left* ear—that is, of the side of the body on which the sympathetic has been cut across. On the animal being allowed to rest in the shade, the reverse and usual state of things obtains again.

The conclusion which the author draws from this experiment is, that the excision of a portion of the sympathetic paralyses agents

capable of producing dilatation of blood-vessels. It can be proved in the same way that the sciatic contains the dilator nerves of the vessels of the paw; for after it has been cut the paw gets less heated on exciting artificial fever.

Ophthalmoscopic Examination during Sleep.

By Dr. J. HUGHING JACKSON.

(*Royal London Ophthalmic Hospital Reports*, 1863.)

Dr. Jackson's reason for examining the eye during sleep, was to help to form some idea as to the condition of the circulation in the brain itself in this physiological condition, the retina and the brain being supplied by branches of the same trunk (the carotid), and these by the same vaso-motor nerves. Dr. Jackson writes:—

"A girl, aged eleven, was admitted into the Hospital for the Epileptic and Paralysed, under the care of Dr. Brown-Séquard, for hemiplegia, which had existed several years. Of this, at the time when the following observations were made, there was little or nothing left, and the child was in fair general health.

"I give the following extracts from my diary as the simplest way of recording several observations:—

"Sept. 3rd.—I tried, first, to examine the eye without using atropine, but the pupil was so small, as is usual in sleep, that I could not illuminate the fundus. I therefore dilated one pupil by atropine, and then examined the fundus of both eyes when the child was awake. I found the optic discs normal. They were equally well coloured, but not abnormally so. I had examined her sight carefully before dropping in the atropine, and found it perfect. When in deep sleep, one pupil was contracted; that dilated by atropine remained enlarged. By the aid of a very intelligent nurse, who held up the upper lid, I was enabled to examine the optic entrance, to which, for the present, I confine my observations. I found that the optic disc was whiter, the arteries a little smaller, and the veins larger than in waking. The veins were thick, and almost plum-coloured. The neighbouring part of the retina also was more anaemic.

"Sept. 6th.—The pupil was now rather small. I saw the optic disc steadily, and could confirm my first statement. The arteries were certainly smaller, and the veins larger than in waking. The other parts of the optic disc were whiter, as was also the neighbouring part of the fundus. She had been well tired by a long romp with the nurse.

"Oct. 3rd.—The pupils had regained their normal size. I again put atropine in the right eye, and examined with the ophthalmoscope. I carefully noted each vessel, especially the smaller ones, and learnt by heart the position and size of both veins and arteries, and also the condition of the optic disc as to colour. At night I examined the eye during sleep. The pupil was smaller than when the child was awake, but I luckily saw well for a long time. The optic disc was not so red, the arteries were certainly smaller, and on this

occasion, I think, the veins were no larger, and about the same as when the child was awake. I then roused her, and examined under similar conditions of light, position, &c. She was awake, but sleepy. I found that the arteries were larger; but, on looking again, I found them smaller as in sleep. They alternated several times. I could not long dwell on the disc; and my opinion is, that the alternation was gradual.

"Oct. 16th.—A girl, aged eleven, a patient under the care of Dr. Ramskill. I dilated the right pupil with atropine. In sleep it remained dilated; the other was contracted. I saw well in this case. The child was deeply asleep, and I had the optic disc under view for a long time. All I can say is, that the disc itself was rather paler in sleep. I roused the child till she was fairly awake, and the only difference then was, that the disc was a little redder.

"The pupil, under the influence of atropine, dilated to the fullest extent when awakened; twice the size it was when the child was asleep. The contraction was not due to the light only. It was the contraction of sleep.

"Oct. 21st.—I put atropine in the right eye, and dilated the pupil to the fullest extent. The child is a somnambulist, and I found her in the ward, at 10 p.m., in the arms of the nurse. The left pupil (the one without atropine) was not so small as usual in sleep. The other was as large as it was when she was awake. She was apparently asleep, however. I examined the eye, and then fairly awakened her, by pinching and making her speak, and getting her to look in certain directions. I again examined her eyes when she had gone to sleep in bed. I feel convinced that the arteries were a little smaller, and the veins larger. I saw well, and for some time.

"Oct. 24th.—Atropine as usual. I saw well; the disc was whiter, and the arteries smaller.

"I ought to observe, that in all these examinations the difference in the size of the arteries and in the coloration and the optic disc during sleeping and waking was but slight."

On a certain Undescribed Voluntary Chest Movement.

By DR. EDWARD SMITH, Assistant Physician to the Hospital for Consumption at Brompton, &c.

(*Dublin Quarterly Journal of Medical Science, August, 1862.*)

In the paper before us, Dr. Smith describes the motions of the throat in two sorts of respiratory acts—those which do not, and those which do involve the closure of the larynx; and to this description he adds some observations upon a voluntary respiratory act, showing that the configuration of the chest may be so changed that nearly the full quantity of air may be admitted and emitted, with variation in the perpendicular diameter only. It is with these observations that we are at present interested:—

"I find," says Dr. Smith, "that with the larynx closed, and

without any air entering the lungs, I can, by the violent action of the thoracic muscles, expand the thorax antero-posteriorly and laterally to the utmost degree, or nearly so, to which it can be expanded in those directions with the admission of air; and that, when it is so expanded, I can admit the same quantity of tidal and complemental air, or nearly so, which could have been admitted in the fullest inspiration without this postural effort. I cannot determine the perpendicular measurement, but I presume that it is lessened. The whole thorax is elevated 1 inch, and there is a decrease of $\frac{1}{4}$ of an inch in the distance from the navel to the upper extremity of the sternum. The figure of the chest with the postural effort, without air, is the same as with ordinary deep inspirations, but there is a greater tucking inwards at the precordium. During the effort the whole body appears to be on the strain, and particularly the muscles of the neck posteriorly and laterally, of the jaw, and of the ribs; and those of the loins are stiff and sore after the exertion. The arms feel as if they were fixed; but it is not so necessarily, for they may be moved about at will. When the posture has been effected, and it is desired to admit the air, there is a peculiar and loud clack in the throat; but this does not take place on the instant, with the will, and seems to imply that the voluntary closure of the throat in preventing inspiration, cannot, on the instant, be removed. The tongue has no essential part in the fixing of the throat referred to, to prevent inspiration, for it may be at rest or extended, and yet it never affects the closing or the opening of the larynx.

"The following are the admeasurements before and after the admission of air, the quantity inspired at each ordinary inspiration being 35 cubic inches, and the movement across the seventh rib being an increase of $\frac{1}{8}$ inch.

"1. *With the postural effort commencing at the normal point of expiration:*—

"Across the seventh rib there is an increase in the circumference to the utmost degree of expansion, without air, of 12-10ths of an inch, and a further increase of 2-10ths of an inch on the admission of air to the utmost degree. Across the nipple the increase without air is 7-10ths of an inch, with a further increase with air of 6-10ths of an inch, and across the second intercostal space the first-mentioned increase is 9-10ths of an inch, and no more with air admitted.

"In reference to the abdomen there is, just below the ribs, a decrease of 6-10ths of an inch, without air, and then an increase of $2\frac{1}{10}$ inches with air admitted. Across the navel the decrease without air is 1 inch, and then the increase with air is 2 inches.

"2. *With the postural effort commencing from the point of deepest expiration:*—

"Across the seventh rib the increase without air is $2\frac{3}{10}$ inches, and another 1 inch with the admission of air. Across the nipple the former is 7-10ths of an inch, and the latter 4-10ths of an inch, whilst across the second space the former is 9-10ths of an inch, and the latter 1-10th of an inch.

"In the abdomen, just below the ribs, there is a decrease of 3-10ths of an inch without air, and then an increase of $3\frac{3}{10}$ inches with

action of the sensitive fibres of the lingual. Therefore also Gluge and Thieresse maintain, as they had already laid down, that sensitive nerve-fibres can never acquire the property of conducting motor impulses.

Cases illustrating the Effects of Nerve-action upon Nutrition and Animal Heat.

By Mr. JONATHAN HUTCHINSON, Surgeon to the London Hospital, &c.

CASE 1.—*Section of the ulnar nerve, with probable wound of the median also—State of the hand three months afterwards—Loss of temperature in the whole hand, and also in the fore-arm above the scar—Dilated and atonic condition of capillaries—Whitlow at the end of the ring finger—Atrophy of the muscles supplied by the ulnar nerve—Recovery of a slight degree of sensation in the parts.*

Emma R.—, aged fifteen, on October 19th, thrust her right hand through a pane of glass, and received a deep cut on the inner part of the fore-arm, about an inch above the wrist-joint. The radial end of the incision passed as far as the middle line of the fore-arm, but possibly it passed deeply still further. It crossed the ulnar vessels and nerve. There was very free bleeding, which was stopped by compress and tight bandage. She was taken to the London Hospital an hour afterwards. No further bleeding occurred. The wound healed slowly and with suppuration. She was discharged five weeks after her admission.

Examination.—February 1st, 1863, rather more than three months after the accident. She is in good health, but has not been able to make any use of her hand. The hand is chilly and bluish red, like that of a person suffering from chilblains; the capillaries fill slowly when emptied by pressure; all the finger nails are somewhat clubbed, and decidedly more curved than those of the other hand. The scar has been quite sound for six weeks, and is free from tenderness. It moves when the fingers are straightened, the tendons being adherent to it. Sensation good above the scar, and exceedingly imperfect below it, over the ulnar side of the hand, the little and ring fingers. No discoverable difference between the two sides of the ring finger. The middle and forefinger have only imperfect sensation; in the thumb it is tolerably good. It appears to improve gradually as we go from the little finger to the thumb. Even over the little finger she has a slight degree of sensation, and can tell sometimes when lightly pricked. All the fingers are bent forward to the palm, but do not touch it. To straighten them causes pain at the scar.

The muscles clothing the metacarpal bone of the thumb are much wasted. The carpo-metacarpal joint is too visible, and the outline of the metacarpal bone is exposed. On the back there is a remarkable hollow between the thumb and forefinger, and the metacarpal bone of the latter is immediately under the skin, the abductor indicis being quite wasted.

When told to draw her thumb across the palm to make it and the little finger meet, she bends the thumb by the long flexor, and is quite unable to abduct it whilst extended. She cannot bring the thumb and little finger together by any effort, partly because she cannot move the little finger at all. The strictures in the palm are a little thickened, and she has slight tenderness on pressure there. She says that she has not had much annoy-

ance from sensation of cold or aching in the hand, but she is quite aware that it is colder than the other; she has also noticed that when the hand is put into hot water she does not feel the heat well.

At the very end of her ring finger (injured hand) is a small whitlow; it looks as if a few drops of pus had been effused beneath the skin, and were now nearly absorbed. The skin over it is dry and horny; there is a little red areola around it. She has had no pain in it, and it is not tender. It began to form nine days ago.

The following is a statement of the temperature as determined by the thermometer, applied to different parts. The experiments were made after the girl had sat in a warm room for half-an-hour, with the two hands equally exposed:—

	Left. Degrees.	Right. Degrees.	Difference. Degrees.
Ulnar borders of the two hands	66	61	5
Between little and ring fingers (in the cleft). . . .	65	59	6
Between thumb and forefinger (in the cleft)	66½	62	4½
Between fore and middle fingers (in cleft)	65	60	5
Pulp of thumb end	62½	60½	2
Ball of thumb (palmar aspect)	65	61	4
Front of fore-arm four inches above wrist (three above the scar)	78	74	4

CASE 2.—Division of the ulnar nerve just above the elbow-joint—Complete and permanent anaesthesia of the integument supplied by it—Inflammation of part of the hand, ending in resolution—Painless ungual whitlow of little finger, with exfoliation of the skin and nail—perfect reproduction of the nail—Contraction of three fingers (unexplained)—Particulars of the state of the arm as to nutrition, animal heat, &c., twenty months after the accident.

[For an opportunity of examining the case recorded in the following notes I was indebted to the kindness of Dr. Powell, the Resident Medical Officer of the Hospital.]

Rose W—, a rather delicate girl, aged fourteen, was brought to me on May 28th, 1863.

In October, 1861 (twenty months ago), she sustained a very severe lacerated cut across the back of her right elbow, by pushing it backwards through a pane of glass. The cut was very deep, and a part of the skin was taken quite away. She was admitted into the London Hospital under Mr. Critchett's care. It was three months before the wound was perfectly closed. With regard to immediate symptoms, she describes most accurately the parts which "were quite numb from the very minute she did it,"—these are those supplied by the ulnar nerve. She states also that there was great swelling of the back of the hand, and that an abscess was feared, but it went away. She had great pain in the middle and ring fingers. There does not appear to have ever been any reason to suspect injury to the elbow-joint itself.

Present Condition.—There is a large supple scar just above the back of the elbow-joint. The whole arm is thinner than the other, the difference being most marked in the lower half of fore-arm. The hand is very much thinner, and about the little finger the muscles are wholly wasted. The metacarpal bone of forefinger is left bare of muscle, and the fleshy mass between it and the thumb is much thinned. The middle, ring, and little fingers are curved into the palm, the ring finger more than the middle one, and the little finger most of all. She cannot straighten them, nor can they by force be bent back into a perfectly straight position. The attempt to do so

gives her no pain, but she says it "drags something at her elbow." Their being curved forwards is not due to paralysis of the extensor, for all the tendons of the latter start up when she tries to extend them, and the first phalanges are bent back on the metacarpal bones, but the other joints remain much curved. The triceps is much wasted, and its tendon has probably been partly cut through on the inner side. The spot where the ulnar nerve has been cut is easily felt. It is a little above the internal condyle. There is a separation of the cut ends for the space of about a quarter of an inch. Both ends are somewhat bulbous, but the lower much the more so, being enlarged to three times the natural size of the nerve. Pressure on these swollen ends does not cause much pain, but she complains of pins and needles if either of them are much pressed, more especially if it is the upper one.

Temperature, Sensation, and Nutrition.—I could not distinguish any positive difference of heat in the parts supplied by the ulnar nerve in the two fore-arms and hands. With the thermometer applied between the little and ring fingers, it appeared that there was about a degree minus on the paralysed side, but this observation was not very positive. There was not the slightest sensation in the little finger, nor ulnar side of ring finger, nor in the integument of the ulnar border of the hand. Sensation was defective, but not wholly absent in the palmar and dorsal aspects of the hand over the metacarpal bones of the ring and little fingers, and also along the ulnar border of lower third of fore-arm. The integument of the affected fingers looked a little redder than that of the opposite hand, but presented no other peculiarity. The end of the little finger looked less than the other, and its nail was not more than half the size of that of the other hand. She told me, in explanation of this, that some two months after the accident, "the nail of this finger came off together with the skin like the end of a glove." She pulled it off one morning, and a new nail afterwards formed. The whole progress of the whitlow had been entirely without pain. I must ask attention to the fact that the new nail—formed on a part wholly deprived of nerve influence (at least of cerebro-spinal)—was perfect in form although not of normal size. The girl also stated that, in consequence of having no feeling in the little finger, she had several times burnt it, and that the sores had always healed well. The day on which I tested the temperature was very warm. Both the girl and her mother assured me that in winter the paralysed parts were usually much colder than the others. I do not at present know how to explain the flexion of the fingers into the palm which so commonly follows section of the ulnar nerve. In most of my cases the tendons of the fore-arm have been injured in the accident, and to their injury I have been inclined to attribute the contraction. Here, however, we have a case in which the nerve was cut high up, and without injury to any other structure, and in which the contraction was most marked. It is interesting also to note that it did not exist at first, but had, according to the patient's statement, gradually come on, and increased during the last six months. She considered that it was still increasing.

Mechanically, it is properly due to contraction of the inner part of the flexor profundus digitorum, aided probably by bending of the wrist by the flexor carpi ulnaris. These structures are supplied by the ulnar nerve, and must now be paralysed. It will be noticed that the contraction is greatest in the case of the little finger, less in the ring, and least of all in the middle one. The fibres of the deep flexor supplying the little finger are wholly dependent upon the ulnar nerve, whilst those more centrally placed received a supply also from the median. The clinical fact that paralysed muscles do sometimes shorten permanently, and even thus more than

counteract their unparalysed opponents, is, I believe, not a new one to those who have paid attention to nerve diseases. It is, however, an exceedingly interesting and difficult subject. At first glance this case would appear to furnish a very definite illustration of it.

On the Vaso-Motor Nerves of the Extremities.

By M. SCHIFF.

(*Comptes Rendus*, t. lv., pp. 400, 462, 540).

More than twenty years ago, Stilling expressed the opinion that the organs of animals belonging to the higher classes were under the influence of three distinct sets of nerves: motor, sensory, and vaso-motor. The latter, according to him, had their origin exclusively in the sympathetic, which trunk, in its action on the vascular system, was completely independent of the spinal cord. He accordingly proposed to substitute for the unmeaning term sympathetic nervous system that of vaso-motor system.

In 1844, Schiff carried on a series of experimental inquiries on the vaso-motor nerves of the mesentery, the liver, and stomach, in order to verify the facts on which this theory is based. He found that these nerves pass through the ganglions of the sympathetic on their way to the spinal cord, and that they go up in the cord as high as the medulla oblongata—some of them, indeed, terminating apparently in the optic thalami. Since then, these facts have been confirmed by a series of experiments made in amphibia and mammalia.

In 1854 and 1855, he published his first researches on the vaso-motor nerves of the hind and fore limbs, the thoracic and abdominal walls of mammalia. He found that they have their origin in the medulla oblongata, and that they traverse the spinal cord, emerging from it with the anterior roots of the spinal nerves. In the course of these researches, he often saw the paralytic dilatation of the vessels, but oftener still noted the increase of temperature. He denies the existence of so-called *calorific* nerves, capable of influencing animal heat independently of blood accumulation. His experiments were performed on animals placed under the influence of chloroform, but his observations were begun several hours only after the anaesthesia had passed off, and were repeated several days in succession and on the same animal. The way in which he proceeded was as follows: he laid open the spinal canal, and divided close to the cord the anterior roots of the nerves which go to form the sciatic trunk. The result which he noted was a considerable and lasting increase of temperature in the foot and the lower one-third or fourth of the leg on the same side. In the rest of the palsied limb temperature and vascularization were normal.

He next cut the roots of the crural nerve, and found no change take place. If, however, the animal moves, the thigh and the upper part of the leg on the paralysed side may be found on examination *colder* than the corresponding parts on the sound side; the reason being that no heat is generated on the palsied limb, as its muscles do not contract. But the trifling difference disappears when

the animal is at rest, whilst the marked elevation of temperature in the paralysed foot remains persistently. The author thus explains away the statement made by another physiologist, that the thighs lose their warmth a few hours after division of the spinal roots of their nerves.

Although this experiment shows that the roots of the sciatic and crural nerves contain the vaso-motor nerves of the foot and the lower part of the leg alone, it does not follow that these roots contain all the vaso-motor nerves of those parts. Some of them go up as high as the lumbar and dorsal portions of the cord, in which is also found the origin of the vascular nerves of the pelvis, the thigh, and the upper two-thirds of the leg and the abdominal parietes.

After the last five, or even the last three, dorsal roots have been divided in a dog without injuring the cord itself, there is always an increase of temperature in the leg and the lower part of the thigh, without any impairment of motion in them. There is very often, also, increased heat in the foot and the upper part of the thigh. But there are cases in which the vascular nerves distributed to the latter regions do not arise from the lower dorsal portion of the cord, but from its upper lumbar segment. This double origin of the vaso-motor nerves of the foot from sacral and lumbo-dorsal roots explains the singular fact that the temperature of the foot, which has been already raised by section of the roots of the sciatic nerve, becomes still more markedly increased by dividing outside the pelvis the paralysed trunk isolated from the cord. The vascular nerves which arise high up have all or in part joined the trunk of the sciatic inside the pelvis.

It may happen, in the case of an animal operated on without chloroform and weakened by loss of blood, that an elevation of temperature in the foot is not produced by section of the roots of the sciatic, whereas division of the nerve outside the pelvis does bring on such an effect, because in the latter case a greater number of vaso-motor nerves are paralysed.

The vascular nerves which arise from the upper lumbar or lower dorsal nerves merely go through the sympathetic on their way to the lower limbs. The sympathetic has no independent influence on the vessels of the lower limbs, but merely acts as a commissure between the trunks of the spinal nerves, conveying to the lower part of the body the nerve-fibres arising from the dorsal roots. These fibres do not all join the crural or sciatic nerve before they are distributed to the lower limb. Those destined for the thigh and leg seem to go directly to the vessels and to accompany their ramifications.

It follows, therefore, that the division of one of the cords of the sympathetic in the lower dorsal or upper lumbar region may give rise to an elevation of temperature over a pretty extensive area in the lower limb. This effect follows without the ganglia being touched, although these are by some considered as independent centres of vaso-motor action.

If a lateral half of the spinal cord be divided in the lumbar region, the same result is obtained as where the roots of the sciatic and crural nerves are cut across. If the section be made above the origin

of the upper root of the crural, near the last dorsal vertebra, the elevation of temperature may affect the *thigh*, in consequence of the upper lumbar roots containing often vaso-motor nerves destined for the thigh.

Section of the cord on a level with the eleventh dorsal vertebra does not completely destroy all voluntary motion in the corresponding lower limb, but from the first there is marked increase of temperature in the thigh, the foot, and toes on the same side.

Section of the cord at about the middle of its thoracic portion is followed by perfectly different results. The foot and lower third of the leg on the same side are always warmer than the same parts on the opposite side, but the knee, thigh, walls of the pelvis, and abdomen retain their normal temperature on the side of the operation, whilst on the opposite side the temperature of those parts is raised. The conclusion to be drawn from this is: that the vaso-motor nerves of the abdominal parietes of the pelvic region of the thigh and the upper two-thirds of the leg *decussate* almost immediately after entering the spinal cord, whilst those of the foot and lower one-third of the leg *do not decussate*.

With regard to the upper extremities, experiments point to an analogous disposition of their nerves. Thus, section of the left lateral half of the cord, at about the middle of the cervical region, is followed by an increase of temperature in the left hand and the lower part of the left fore-arm, but the rest of the *left* limb retains its normal temperature, whilst the same portions of the *right* limb are warmer. At the same time the foot and the region near the malleoli are warmer on the left side, but the knee, the thigh, and the trunk feel warmer on the right side.

The author was not able to determine the exact spot where simple section of one-half of the cord raises the temperature of the whole corresponding upper limb, and he concludes that the vaso-motor nerves of the upper extremities decussate immediately on entering the cord.

Section of the left lateral half of the medulla oblongata is followed by an elevation of temperature limited on the left side to the head, the hand, the foot, the lower part of the fore-arm and leg; on the *right* side, to the trunk, the humerus, the thigh, the elbow, the knee, the upper part of the fore-arm and leg. This experiment shows that the vaso-motor nerves go up to the medulla oblongata, and that the nerves of the head, the feet, and the hands do not decussate at all. These nerves do not extend higher up than the medulla oblongata, for section of one lateral half of the pons varolii causes no difference of temperature in the extremities of either side of the body.

The vaso-motor nerves of the liver and the stomach, however, seem to ascend as high as the optic thalami. "Now it is a remarkable fact," says the author, "that in fever, apart from the general increase of temperature of the blood, the local increase of heat is chiefly felt in those regions that are supplied by non-decussating vascular nerves—namely, in the face, the hand, the foot, and part of the fore-arm and leg. These nerves, then, seem to form a distinct group."

The author next considers the influence of reflex action on vaso-motor nerves. There are, he says, two ways in which these nerves may act on blood-vessels :—they may either *constrict* them through existing contraction of their circular muscles, or they may *dilate* them. Active dilatation has been denied, and has been looked upon as the result of a paralysis of the constrictor muscles of the vessels, or of exhaustion of these muscles, due to a constriction which always precedes dilatation. But M. Schiff has performed experiments which prove—1st. That the vascular dilatation which follows as an effect of irritation is not always preceded by contraction of the vessel, and that there are therefore vascular dilatations which are not due to exhaustion of the circular fibres. 2nd. That the dilatation is not a mechanical result of an augmented flow of blood ; for if there were such a flow of blood, the dilatation would not be limited to the point irritated, but would be still more marked in the more central portion of the artery situated below the irritated spot. 3rd. That the dilatation is not a mechanical effect resulting from a reflex contraction situated either further on, near the periphery or in the venous system ; for such a contraction should exist either immediately above the point irritated in the continuity of the vessel (in which case it would be seen), or it would be seated in a more distant spot (in which case it should cause a dilatation of the vessel in its whole length, and not in one limited part alone). 4th. That the sympathetic contains no vascular nerves which preside over this kind of dilatation.

Dilatation produced by tickling does not take place independently of the nerves, for no such result follows after all the sensitive nerves of the part have been divided. The author regards it as a local effect of a local irritation, occurring independently of a general disturbance of the circulation. It is not passive, and must therefore be an active phenomenon. It is under the influence of the nervous system, and yet the presence of nervous trunks is not an indispensable condition of its production. In this respect it is analogous to muscular contraction, and it further resembles it in that the irritation must be increased after the nervous trunks have been cut off.

The following experiment now proves that active vascular dilatation is also dependent on a motor-nervous system which directly provokes it. The cervical portion of the left sympathetic is excised in a dog ; for many successive days or weeks after this, the left side of the head and the left ear are found warmer than the right, where the vessels are a little more dilated. If now the animal be taken out in the fields, he gets warmer all over ; both ears participate in this increase of temperature, their blood-vessels dilate a little without any previous stage of contraction. At last when the animal pants for breath, it is noticed that the blood-vessels of the *right* ear are more dilated than those of the *left* ear—that is, of the side of the body on which the sympathetic has been cut across. On the animal being allowed to rest in the shade, the reverse and usual state of things obtains again.

The conclusion which the author draws from this experiment is, that the excision of a portion of the sympathetic paralyses agents

capable of producing dilatation of blood-vessels. It can be proved in the same way that the sciatic contains the dilator nerves of the vessels of the paw; for after it has been cut the paw gets less heated on exciting artificial fever.

Ophthalmoscopic Examination during Sleep.

By Dr. J. HUGHLING JACKSON.

(*Royal London Ophthalmic Hospital Reports*, 1863.)

Dr. Jackson's reason for examining the eye during sleep, was to help to form some idea as to the condition of the circulation in the brain itself in this physiological condition, the retina and the brain being supplied by branches of the same trunk (the carotid), and these by the same vaso-motor nerves. Dr. Jackson writes:—

"A girl, aged eleven, was admitted into the Hospital for the Epileptic and Paralysed, under the care of Dr. Brown-Séquard, for hemiplegia, which had existed several years. Of this, at the time when the following observations were made, there was little or nothing left, and the child was in fair general health.

"I give the following extracts from my diary as the simplest way of recording several observations:—

"Sept. 3rd.—I tried, first, to examine the eye without using atropine, but the pupil was so small, as is usual in sleep, that I could not illuminate the fundus. I therefore dilated one pupil by atropine, and then examined the fundus of both eyes when the child was awake. I found the optic discs normal. They were equally well coloured, but not abnormally so. I had examined her sight carefully before dropping in the atropine, and found it perfect. When in deep sleep, one pupil was contracted; that dilated by atropine remained enlarged. By the aid of a very intelligent nurse, who held up the upper lid, I was enabled to examine the optic entrance, to which, for the present, I confine my observations. I found that the optic disc was whiter, the arteries a little smaller, and the veins larger than in waking. The veins were thick, and almost plum-coloured. The neighbouring part of the retina also was more anaemic.

"Sept. 6th.—The pupil was now rather small. I saw the optic disc steadily, and could confirm my first statement. The arteries were certainly smaller, and the veins larger than in waking. The other parts of the optic disc were whiter, as was also the neighbouring part of the fundus. She had been well tired by a long romp with the nurse.

"Oct. 3rd.—The pupils had regained their normal size. I again put atropine in the right eye, and examined with the ophthalmoscope. I carefully noted each vessel, especially the smaller ones, and learnt by heart the position and size of both veins and arteries, and also the condition of the optic disc as to colour. At night I examined the eye during sleep. The pupil was smaller than when the child was awake, but I luckily saw well for a long time. The optic disc was not so red, the arteries were certainly smaller, and on this

occasion, I think, the veins were no larger, and about the same as when the child was awake. I then roused her, and examined under similar conditions of light, position, &c. She was awake, but sleepy. I found that the arteries were larger; but, on looking again, I found them smaller as in sleep. They alternated several times. I could not long dwell on the disc; and my opinion is, that the alternation was gradual.

"Oct. 16th.—A girl, aged eleven, a patient under the care of Dr. Ramskill. I dilated the right pupil with atropine. In sleep it remained dilated; the other was contracted. I saw well in this case. The child was deeply asleep, and I had the optic disc under view for a long time. All I can say is, that the disc itself was rather paler in sleep. I roused the child till she was fairly awake, and the only difference then was, that the disc was a little redder.

"The pupil, under the influence of atropine, dilated to the fullest extent when awakened; twice the size it was when the child was asleep. The contraction was not due to the light only. It was the contraction of sleep.

"Oct. 21st.—I put atropine in the right eye, and dilated the pupil to the fullest extent. The child is a somnambulist, and I found her in the ward, at 10 p.m., in the arms of the nurse. The left pupil (the one without atropine) was not so small as usual in sleep. The other was as large as it was when she was awake. She was apparently asleep, however. I examined the eye, and then fairly awakened her, by pinching and making her speak, and getting her to look in certain directions. I again examined her eyes when she had gone to sleep in bed. I feel convinced that the arteries were a little smaller, and the veins larger. I saw well, and for some time.

"Oct. 24th.—Atropine as usual. I saw well; the disc was whiter, and the arteries smaller.

"I ought to observe, that in all these examinations the difference in the size of the arteries and in the coloration and the optic disc during sleeping and waking was but slight."

On a certain Undescribed Voluntary Chest Movement.

By Dr. EDWARD SMITH, Assistant Physician to the Hospital for Consumption at Brompton, &c.

(*Dublin Quarterly Journal of Medical Science*, August, 1862.)

In the paper before us, Dr. Smith describes the motions of the throat in two sorts of respiratory acts—those which do not, and those which do involve the closure of the larynx; and to this description he adds some observations upon a voluntary respiratory act, showing that the configuration of the chest may be so changed that nearly the full quantity of air may be admitted and emitted, with variation in the perpendicular diameter only. It is with these observations that we are at present interested:—

"I find," says Dr. Smith, "that with the larynx closed, and

without any air entering the lungs, I can, by the violent action of the thoracic muscles, expand the thorax antero-posteriorly and laterally to the utmost degree, or nearly so, to which it can be expanded in those directions with the admission of air; and that, when it is so expanded, I can admit the same quantity of tidal and complementary air, or nearly so, which could have been admitted in the fullest inspiration without this postural effort. I cannot determine the perpendicular measurement, but I presume that it is lessened. The whole thorax is elevated 1 inch, and there is a decrease of $\frac{1}{4}$ of an inch in the distance from the navel to the upper extremity of the sternum. The figure of the chest with the postural effort, without air, is the same as with ordinary deep inspirations, but there is a greater tucking inwards at the precordium. During the effort the whole body appears to be on the strain, and particularly the muscles of the neck posteriorly and laterally, of the jaw, and of the ribs; and those of the loins are stiff and sore after the exertion. The arms feel as if they were fixed; but it is not so necessarily, for they may be moved about at will. When the posture has been effected, and it is desired to admit the air, there is a peculiar and loud clack in the throat; but this does not take place on the instant, with the will, and seems to imply that the voluntary closure of the throat in preventing inspiration, cannot, on the instant, be removed. The tongue has no essential part in the fixing of the throat referred to, to prevent inspiration, for it may be at rest or extended, and yet it never affects the closing or the opening of the larynx.

"The following are the admeasurements before and after the admission of air, the quantity inspired at each ordinary inspiration being 35 cubic inches, and the movement across the seventh rib being an increase of $\frac{1}{8}$ inch.

"1. With the postural effort commencing at the normal point of expiration:—"

"Across the seventh rib there is an increase in the circumference to the utmost degree of expansion, without air, of 12-10ths of an inch, and a further increase of 2-10ths of an inch on the admission of air to the utmost degree. Across the nipple the increase without air is 7-10ths of an inch, with a further increase with air of 6-10ths of an inch, and across the second intercostal space the first-mentioned increase is 9-10ths of an inch, and no more with air admitted.

"In reference to the abdomen there is, just below the ribs, a decrease of 6-10ths of an inch, without air, and then an increase of $2\frac{9}{10}$ inches with air admitted. Across the navel the decrease without air is 1 inch, and then the increase with air is 2 inches.

"2. With the postural effort commencing from the point of deepest expiration:—"

"Across the seventh rib the increase without air is $2\frac{3}{10}$ inches, and another 1 inch with the admission of air. Across the nipple the former is 7-10ths of an inch, and the latter 4-10ths of an inch, whilst across the second space the former is 9-10ths of an inch, and the latter 1-10th of an inch.

"In the abdomen, just below the ribs, there is a decrease of 3-10ths of an inch without air, and then an increase of $3\frac{3}{10}$ inches with

admitted air, and across the navel a decrease of 8-10ths of an inch without air, and an increase of 2 inches with air.

" The quantity of air admitted without increasing the size of the thorax beyond that of the fixed posture under discussion is 180 cub. inches, and that to the utmost capacity of the chest is 210 cub. in., the starting point being that of ordinary inspiration. When, however, the effort is made from the point of most complete inspiration, the quantity of air admitted is 230 cub. inches, without increasing the size, and 260 cub. inches when expanded to the utmost degree.

" The admeasurement and quantities vary somewhat with the fatigue, for it is impossible to maintain for a long time, or to repeat often, the exertion without fatigue. It is also imperative to have the movements most perfectly under the control of the will.

" Thus in the postural efforts a very great increase in the admeasurement of the chest is effected, without the admission of air, and a slight further increase on the admission of air; whilst the circumference of the abdomen is lessened without the admission of air, and greatly increased afterwards on its admission. But as much as 80 cub. inches of air may be admitted into the lungs before the circumference of any part of the abdomen enlarges. When the starting-point is that of complete expiration the chest admeasurements are increased, whilst those of the abdomen are decreased, before air is admitted, and a proportionate increase in the size of the abdomen results after air has been admitted. The direction of the enlargement of the chest by the inspired air is downwards.

" It is thus evident that complete inspiration may be effected by expansion of the lungs in one direction only, but with regard to expiration that is either impossible or very difficult under the same circumstances. While inspiration may be thus as quickly and as surely effected as under ordinary conditions, the expirations must be made very slowly, and unless a much longer period is allowed for its performance than under ordinary circumstances, the whole of the complementary and tidal, and part of the reserve air, is not emitted, and, as a consequence, less air is admitted during the following inspiration. Hence I think it is proved that inspiration may be completely effected by the diaphragm alone, and that the attendant expansion of the thorax is not essential, provided it be expended at the period of inspiration, but that expiration cannot be effectually performed by the abdominal muscles alone, so long as the thorax remains expanded; and hence that the respiration would be seriously impeded, if from any cause the thorax remained expanded beyond the normal degree.

" I do not know if the capacity of the chest is varied in either direction by this postural effort, since it is impossible to determine on myself the perpendicular depth of the chest. There is no doubt, I believe, that the capacity of the chest may be enlarged to a certain extent, without the admission of air, by the expansion of the contained air; and the conditions under which the air is placed during this effort, are very favourable to its expansion.

" There must be a dislocation of the air within the lungs, so as to permit of the change of figure of the lungs; and hence the air-cells

in parts of the lungs must be at the same moment in two opposite conditions—viz., those at the base containing as little air as possible, whilst those centrally, laterally, and superiorly situated must be distended either to their utmost extent, or to the utmost capacity of the chest at those points. Hence it is proved that an empty condition, or nearly so, of the air-cells, is not necessarily due to the pressure of the thoracic walls, as a whole, and that, in fact, partial collapse on a large scale may occur; and, in like manner, that distension of a great part of the air-cells may occur without the entrance of air from without, and simply by dislocation of that within the lungs.

" It is also clear that whilst the air cells of the greater part of the thorax are so distended that they cannot permit the entrance of any further quantity of air, those at the base are capable of receiving 260 cubic inches—or, in fact, the whole quantity which the lungs can inspire from the point of most complete expiration; and hence that the abolition of a large part of the air cells at the apex could not prevent the introduction into the lung of the full volume of air required, provided that the cells at the base could be first sufficiently emptied, and that the requisite amount of muscular power and space in the abdomen could be obtained.

" It may also be inferred, that if it be possible to keep the air cells of so large a part of the lungs permanently distended beyond the natural degree, there would be a more limited interchange of gases; and it would be of interest to determine the effect which this would have upon the chemical changes and the circulation of blood in the lungs. Hence, in cases of disease in which the mobility of the chest is lessened, it is a desideratum to determine whether that results from the retention of an unusual quantity of air, or simply from an absolutely lessened capacity to contain air.

" Such are the results of this inquiry on the two subjects indicated; and whilst I must leave the practical application of them to the physician, in his investigations of chest and throat disease, and to the physiologist, in studying the mechanism of respiration, I would conclude by directing attention to the great diversity which exists on these two subjects in the most widely-spread classes of chest disease—bronchitis and phthisis.

" In old bronchitis we find the cavity of the fauces large, the mucous membrane more or less suffused and thickened, and the movements of the throat materially lessened; while in phthisis there is commonly a narrowed state of the fauces, attenuation of the lining membrane, and particularly of the arches of the palate, with almost obliteration of the anterior arch, and with perfect and free movement of the throat.

" In old bronchitis the chest is fixed at the upper and lateral parts, and the abdomen tucked in, *as in the position of the chest which has been discussed*, rendering the expiration of air extremely difficult, and diminishing greatly the interchange of air. This fixness of the thoracic walls often extends even to the lowest ribs, leaving the respiration to be performed almost entirely in the direction of the perpendicular axis of the chest. In phthisis there is no fixing of the chest by violent muscular effort; and the cause

of the respiration being carried on chiefly at the base is due simply to the closure of the lungs at their upper part.

"Hence it is impossible not to notice how completely the cause of the dyspnœa differs in the two conditions, how greatly is the respiratory act lessened in bronchitis, and how much the diminution is due to an abnormal action of the inspiratory chest muscles, whereby the collapse of the chest is prevented during expiration, and the expiration greatly impeded—a condition evidently greatly connected with cerebro-spinal action, and to be treated on that indication much more than has hitherto been practised."

On the Nature and Cause of the Respiratory Murmur.

By Dr. HYDE SALTER, Assistant-Surgeon to the
Charing-cross Hospital, &c.

(*Medico-Chir. Review*, January, 1864.)

The conclusions at which Dr. Salter arrives in this paper are :—

1. That the air-cells are structurally incapable of producing a respiratory murmur by their slight dilatation.
 2. That the respiratory murmur is, immediately and essentially, a fine tube sound.
 3. That the lobular passages and ultimate bronchial radicles are probably its immediate seat.
 4. That while the lung parenchyma, from its heterogeneous constitution, completely muffles all deep-seated sound, the unbroken column of air in the bronchial tree is an excellent conductor; that thus the respiratory murmur has a double cause, and is in part the result of air-and-wall friction at the spot, and in part the bronchial sound of the larger tubes (although essentially modified) conducted by the unbroken column of air to the ultimate bronchial twigs.
 5. That the restriction of the sound to inspiration (the "excess of inspiratory over expiratory sound" of authors) depends mainly on convection, but probably also in part on the great amount of air-and-wall friction produced on the impact of the air at the points where the tubes divaricate.
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Experiments on the Uses and Destination of Food.

By Mr. WILLIAM S. SAVORY.

(*Proc. of Royal Society*, May 22, 1862.)

The experiments which are related in this paper refer to the destination of food and its uses.

Abundant, nay, superfluous evidence has been furnished to prove that no one principle of food will alone suffice for nutrition; but clear and unequivocal evidence is still wanting to show how far each principle of food is essential to life and health, provided all else save that one be sufficiently supplied. This is a very different question.

Again, ever since Liebig's famous classification of food into plastic or nutritive and respiratory or calorifacient, some most important questions in connexion with it have engaged the attention of physiologists. Amongst them are these :—

Is any food destined to the production of heat without being concerned in the repair of the tissues—that is, is any portion of the food directly burnt in the blood?

Is any portion of albuminous food directly calorifacient, that is, burnt in the blood without forming tissue?

This last question has more recently assumed another form, viz., what is the source of urea? Is it derived wholly from the metamorphosis of tissue, or directly to some extent from the blood? In other words, does any portion of nitrogenous food undergo a directly retrograde metamorphosis into urea, carbonic acid, and water?

The experiments were performed upon rats and a hawk. The animals were fed upon different diets, and the experiments may be divided into three classes accordingly. In one class the diet was a non-nitrogenous one, consisting of equal parts by weight of arrow-root, sago, tapioca, lard, and suet; for this mixture was found upon analysis to yield only 22 per cent. of nitrogen. In another class the diet was a nitrogenous one. It consisted of lean veal, from which every visible particle of fat had been carefully removed. This yielded upon analysis only 1.55 per cent. of fat. In the third class the diet was a mixed one. It consisted of a combination of the two former diets.

The weight, temperature, and general condition of the animals were especially noticed, and in some cases the urine was collected and the amount of nitrogen it contained determined.

From these experiments the following conclusions are drawn :—

Nitrogenous materials are not only calorifacient, but, at least under some circumstances, sufficiently so to maintain alone the requisite temperature.

It is in the highest degree probable that, under certain circumstances, nitrogen materials may prove directly calorifacient without forming tissue.

Non-nitrogenous substances are, at least under some circumstances, directly calorifacient without entering into the composition of tissue of any kind.

While non-nitrogenous food only is taken, all the nitrogen which is excreted in the urine, and more, may be accounted for by the disintegration of the original tissues, without assuming that any fraction is assimilated from any other source.

While life cannot be maintained without nitrogenous food, even though every other kind be abundantly supplied, death in this case being due to loss of tissue, life and even health and the normal temperature can be maintained, at least for a long period, upon a diet almost exclusively nitrogenous, with proper inorganic substances in which there exists only a small fraction of non-nitrogenous matter. Such a minute proportion of fat must be but a poor representative of non-nitrogenous food.

Moreover in these experiments some of the rats sustained a loss of weight considerably above 50 per cent.

The difference in this respect between former experiments and mine may be, perhaps, in some measure accounted for by considering the immediate cause of death in the former ones. Chossat satisfactorily showed that the subjects of his experiments died from cold. In my experiments, the animals being freely supplied with calorificient food, this cause of death was for awhile averted, so that time was allowed for a further disintegration of tissue.

When their temperature is maintained from external sources, or when they are freely supplied with calorificient food, warm-blooded animals may die rather from waste than loss of temperature, as perhaps is the case with cold-blooded animals when they are starved.

Lastly, in these experiments the significant fact appeared, that while the weight, strength, and general condition of the animals varied very widely under the different diets to which they were subjected, no considerable fluctuation was observed in their temperature. Even the slight variation from time to time recorded seemed rather to result from other causes than to depend directly on the food.

*On the Properties of the Saliva of the Parotid and
Submaxillary Gland in Man.*

By Dr. BEALE, Professor of Anatomy in King's Coll., London.

(*Beale's Archives of Medicine*, January, 1863.)

The author gives, as the result of his inquiries, the following conclusions:—“1. The saliva of the submaxillary gland is always alkaline, that of the parotid in most cases. Sometimes, especially in the morning, the first few drops of the latter are found of an acid reaction; soon, however, this disappears, and an alkaline one takes its place. 2. Immediately after secretion each kind of saliva is perfectly transparent and fluid, and does not contain any cells. Very seldom, however, a few epithelial cells of the ducts are met with; but the corpuscles, so peculiar to the mixed saliva of the mouth, are never found. After a while the fluid of the submaxillary gland gets gradually viscous, and a kind of flocculent sediment is found; whereas the parotid saliva does not change at all, or at most gets a little opaque. 3. Both kinds become opaque when heated, and even small quantities of coagulated matter are formed. By adding mineral acids, solution of the precipitate does not take place. But the quantity of this substance is too small for determining its amount or ascertaining its properties. We shall see that this principle takes part in the physiological action of the saliva. 4. The specific gravity of the parotid fluid is higher than that of the submaxillary gland; and as to the aqueous residue, we find that the same holds. 5. In man each kind of saliva converts starch into sugar. As to this point there is a remarkable difference between the saliva of man and dog. In the latter, according to the investi-

gations made by MM. Schmidt and Bidder, it is only the mixture of the different fluids of the glands with the fluid of the mucous membrane of the mouth which is able to produce the change just mentioned. In man there is no doubt that the saliva of the submaxillary glands, as well as that of the parotid, acts upon the starch within a very short time. I have often tried it with the same result. 6. Both fluids have this property when heated for a short time, whereas refrigeration to some degrees below zero of the thermometer does not injure them in any way. Remembering the fact 3, we may submit that the conversions of amyloid substances into sugar might be effected by the small quantity of albuminous matter we find in each kind of saliva. 7. Sulphocyanide of potassium I found only in the saliva of the parotid gland. In that of the submaxillary gland I failed absolutely, although I tried it at different times and on different individuals."

On the Indigestibility of the Living Stomach in its own Secretion.

By Dr. PAVY, Lecturer on Physiology at Guy's Hospital, &c.

(*Proc. of the Royal Society*, Jan. 8, and May 7, 1863.)

The author referred to the communication by John Hunter 'On the Digestion of the Stomach after Death,' published in the *Philosophical Transactions* for 1772. In this communication Hunter notices that in occasional instances, especially in persons who have died of sudden and violent deaths, the stomach is found on inspection to have undergone solution, to the extent of perforation, from the action of its own secretion upon it. Hunter considered that this could only have taken place after death; and to explain why the same occurrence did not ensue during life, he adduced the living principle as constituting the protecting agent. The fact that parts of living animals, as shown by Claude Bernard of Paris, are susceptible of digestion when introduced through a fistulous opening into a digesting stomach, proved that Hunter's explanation does not stand the test of experiment. The author corroborated Bernard's results upon frogs, and referred to an experiment in which he had also obtained the digestion of the extremity of the ear of a living rabbit.

The view at present most generally entertained is, that the epithelial lining or mucus protects the stomach from undergoing digestion during life. This, it is supposed, is acted upon and dissolved, but being as constantly renewed, the stomach escapes injury. There being no longer the power of producing epithelium after death, accounts for the occurrence of the solution that may then be observed.

To test this view, the author removed a patch of mucous membrane about the size of a crown-piece from the stomach of the dog. Food was afterwards digested without, however, the denuded stomach showing the slightest sign of attack. It thus appearing

that the stomach resisted digestion notwithstanding the assumed protecting layer had been removed, it became evident that something besides the epithelial lining was required to account for the security enjoyed.

Seeing that the question was still open for explanation, the following was the view propounded by the author. The existence of acidity, it was first remarked, is an absolutely essential condition for the accomplishment of the act of digestion. During life the walls of the stomach are most freely permeated by a current of alkaline blood. Under such circumstances it would appear impossible that any digestive action could be effected. There would be one condition that would neutralize the other. Acidity is needful for digestion, and alkalinity is a constant character of the blood. As long, therefore, as so free a circulation of this alkaline fluid should be maintained (and this happens to be one of the necessary conditions of life), the stomach will be supplied with a source of protection competent to afford it the security from attack by its own secretion that it enjoys.

Digestion of the stomach may be effected after death, because the blood, being then stagnant, is incapable of offering the barrier produced by a circulating current.

Experiments were mentioned in which the circulation through the stomach had been arrested during life so as to imitate the condition, as far as the stomach was concerned, that exists after death. Although this was effected whilst the process of digestion was actively proceeding, yet it was only in some cases that the mucous membrane of the stomach was attacked. On repeating the experiment, however, having previously introduced a dilute non-corrosive acid (the phosphoric and citric were the acids employed) into the stomach, the result was solution and perforation in a short space of time.

The author had expected, when he commenced his experiments, to have obtained the same result upon arresting the circulation through the stomach as occurs after death; but it became evident to him on reflection that although the circulation through the stomach may be stopped by ligatures during life, yet the conditions are not thereby rendered completely identical with those prevailing after death. There is still a circulation all around the stomach, and from the facility with which the permeation of fluids takes place, a certain amount of counteractive influence would still be exerted. By the artificial introduction, however, of an acid into the cavity of the stomach before its vessels were ligatured, the surrounding circulation became inadequate to afford the required neutralizing power, and perforation therefore quickly resulted.

It did not appear to the author that the digestion of the living tissues of animals referred to in the first part of his paper formed any valid objection to his view. In the case of the frog's legs, he considered it might be fairly taken that the amount of blood possessed by the animal would be inadequate to furnish the required means of resistance. In the case of the rabbit's ear, the vascularity of the part being so much less than that of the walls of the stomach, he

thought there was nothing unreasonable in conceiving that, whilst the one might receive protection through the circulating alkaline current, the other might be unable to resist attack. There was no comparison between the position of the stomach and that of the rabbit's ear, and the question, according to his view, resolved itself into degree of power possessed by the acidity of the contents of the stomach on the one hand, and the alkalinity of the circulating current on the other.

The author concluded by adducing experimental evidence to show that pepsine was contained in the walls of the stomachs of persons who had died from severe diseases, as well as in the normal fasting and digesting stomach.

This is the abstract of the paper read at the Royal Society 8 June, 1863. What follows is the abstract of a sequel to this paper read in the same place, 7 May, 1863 :—

Upon much further extended observation the author found that the standard he had taken from the rabbit, as regarded the *post-mortem* action of the contents of the stomach upon the organ itself, was not just in its application to the dog. Actual experiment on the dog had shown that upon the animal being killed at a period of full digestion, and its temperature being afterwards maintained about the degree belonging to life, the effect at the end of five and six hours only amounted to more or less digestion of the mucous membrane. In the rabbit, under similar circumstances, the effect had gone on to perforation, on account, apparently, of the stronger acidity of the gastric contents. In reality, then, the effect of arresting the circulation through the stomach during life, about coincided in both rabbit and dog with what occurred, other circumstances being equal, after death.

As a counterpart to the experiments originally mentioned, where dilute non-corrosive acids had been introduced into the stomach of the dog, and the flow of blood through the organ afterwards arrested, an operation that was followed by comparatively rapid perforation, the author had used the same acids, in the same quantities, and similarly diluted, but the circulation was allowed to remain free, and now the stomach resisted digestive attack. Ligatures had of course been applied to secure the retention of the acid liquid introduced.

A mode of experimenting suggested by Dr. Sharpey had been undertaken. After an incision through the anterior wall of the stomach, a portion of the posterior wall had been drawn forward, and a ligature placed tightly around it, so as thus to arrest the circulation through a limited portion of the organ's parietes. It was found that this constricted mass underwent digestion like a morsel of food.

An experiment had been performed bearing on the explanation that had been given to account for the attack upon the living frog's legs and rabbit's ear by digestion whilst the stomach remained protected. Three drachms of muriatic acid, diluted to three ounces with water, were introduced into the stomach of a dog, and the end of the oesophagus and the pylorus ligatured, without including the

vessels, so that the circulation through the organ was left free. In one hour and forty minutes death took place, and on the parts being examined immediately, perforation, with extensive digestion of the interior of the stomach throughout, was found. The author considered that the question of result was clearly shown to resolve itself into one dependent on degree of power possessed by the acid contents of the stomach on the one hand, as against the alkaline circulation on the other. With a certain amount of acid only in the stomach, the circulation can afford the required protection; whilst with a larger amount the influence of the acid prevails, and digestive solution of the organ is the result. Allow, now, the contents of the stomach to remain the same, and vary the degree of vascularity in the parts submitted to the digestive influence. We have simply here a converse arrangement of the circumstances; and the position is represented by the situation of the stomach as compared with that of the frog's legs and rabbit's ear."

Experimental Researches into a New Excretory Function of the Liver, consisting in the Removal of Cholesterine from the Blood, and its Discharge from the Body in the shape of Stercorine (the Seroline of Boudet).

By Dr. AUSTIN FLINT, Jun., Professor of Physiology in
Bellevue Hospital College, New York.

(*American Journal of the Medical Sciences*, Oct. 1862.)

This paper is the record of a very elaborate experimental inquiry. The general result is as follows:—

1. The cholesterine exists in the bile, the blood, the nervous matter, the crystalline lens, and the meconium; but is not found in the faeces in ordinary conditions. The quantity of cholesterine in the blood of the arm is from five to eight times more than the ordinary estimate.
2. Cholesterine is formed in great part, if not entirely, in the substance of this nervous matter, where it exists in great abundance; from which it is taken up by the blood, and constitutes one of the most important of the effete or excrementitious products of the body. Its formation is constant; is always existing in the nervous matter and circulating fluid.
3. Cholesterine is separated from the blood by the liver, appears as a constant element of the bile, and is discharged into the alimentary canal. The history of this substance in the circulating fluid and in the bile, marks it as a product destined to be got rid of by the system, or an excretion. It pre-exists in the blood, subserves no useful purpose in the economy; is separated by the liver, and not manufactured there; and if this separation be interfered with, accumulates in the system, producing blood-poisoning.
4. The bile has two separate and distinct functions dependent on the presence of two elements of an entirely different character. It has a function connected with nutrition. This is dependent on the presence of the glyco-cholate

and tauro-cholate of soda, which do not pre-exist in the blood, subserve a useful purpose in the economy, and are not discharged from it, are manufactured in the liver and peculiar to the bile, do not accumulate in the blood when the function of the liver is interfered with, and are, in short, products of secretion. But it has another function connected with depuration which is dependent on the presence of cholesterine which is an excretion ; the flow of the bile is remittent, being much increased during the digestive act, but produced during the intervals of digestion for the purpose of separating the cholesterine from the blood, which is constantly receiving it. 5. The ordinary normal faeces do not contain cholesterine, but contain *stercorine* (formerly called seroline, from its being supposed to exist only in the serum of the blood) produced by a transformation of the cholesterine of the bile during the digestive act. 6. The change of cholesterine into stercorine does not take place when digestion is arrested, or before this process commences ; consequently stercorine is not found in the meconium or in the faeces of hibernating animals during their torpid condition. These matters contain cholesterine in large abundance, which also sometimes appears in the faeces of animals after a long fast. Stercorine is the form in which cholesterine is discharged from the body. 7. The difference between the two varieties of jaundice with which we are familiar, are characterized only by yellowness of skin, and are comparatively innocuous, while the other is attended with very grave symptoms, and is almost invariably fatal, and is dependent upon the obstruction of the bile in the one case, and its suppression in the other. In the first instance, the bile is confined in the excretory passages, and its colouring matter is absorbed, while in the other the cholesterine is retained in the blood and acts as a poison. 8. There is a condition of the blood dependent upon the accumulation of cholesterine, which Dr. Flint has called *Cholesteræmia*. This only occurs when there is a structural change in the liver, which incapacitates it from performing its excretory functions. It is characterized by symptoms of a grave character, referable to the brain, and dependent upon the poisonous effects of the retained cholesterine on this organ. It occurs with or without jaundice. 9. Cholesteræmia does not occur in every instance of structural disease of the liver. Enough of the liver must be destroyed to prevent the due elimination of the cholesterine. In cases in which the organ is but moderately affected the sound portion is capable of performing the eliminative function of the whole. 10. In cases of simple jaundice, when the faeces are decolorized and the bile is entirely shut off from the intestine, stercorine is not formed in the evacuations ; but in cases of jaundice with cholesteræmia, the stercorine may be found, though always very much diminished in quantity, showing that there is an insufficiency in the separation of the cholesterine from the blood, though its secretion is not easily suspended. After death in these cases, but a small quantity of bile is found in the gall-bladder.

On Amyloid Substance of the Liver, and its ultimate Destination in the Animal Economy.

By Dr. ROBERT McDONNELL.

(Proc. of the Royal Society, March 12, 1863.)

After briefly referring to the discovery of the amyloid substance of the liver, and the earlier history of the subject, the author examines the facts which have induced Dr. Pavy to conclude that this substance is not normally transformed into sugar during life. The author being led, after a careful repetition of Dr. Pavy's experiments, to concur in his views, asks, If then the amyloid substance of the liver be not converted into sugar, what becomes of it? what is its normal destination in the animal economy? It is the object of the memoir to attempt to answer this question, which, it must be admitted, is one of the greatest delicacy; nevertheless there appears on the whole to be evidence that the amyloid substance met with in the liver is on its way upwards towards the more exalted or complex immediate animal principles; that, in fact, the process of healthy assimilation tends, if the expression may be used, to promote it from the rank of ternary (hydrocarbonous) to that of quaternary (azotized) compounds; and that its conversion into sugar is to be looked upon as a deviation from this progressive course—a dissimilative instead of an assimilative process. In order to establish this view it became necessary—

1st. To investigate the chemical and physiological relations of the amyloid substance, not only of the liver, but of other organs and tissues, and to test the very interesting results, which are for the most part due to M. Charles Rouget.

2ndly. To compare the portal and hepatic blood with each other, and with arterial and venous blood derived from other sources; and

3rdly. To consider the relations to each other of the different functions performed by the liver. For if it be true, as Lehmann, Brown-Séquard, and others, have asserted, that the fibrin and much of the albumen of the portal blood vanishes in the liver, and that at the same time that it destroys these azotized compounds it forms its non-azotized amyloid substance, and excretes bile containing so little nitrogen that it need hardly be taken into account, are we not, from the consideration of these functions, led to infer that the nitrogen which leaves the liver by no other outlet may go forth in the hepatic blood in union with the amyloid substance thus changed into a new azotized principle;—that thus the liver is a great blood-making organ, in which there is constantly going on a reconstruction of certain ingredients of the blood; that in it the fibrin, &c., which has done its work, is disintegrated, the hydrocarbons of the bile abstracted, and the nitrogen combined with the amyloid substance, which, instead of being normally changed into sugar, emerges from the liver a constituent principle of the protoplasma, from the bosom of which (to use the words of Bernard with reference to the foetal tissues) organic evolution is to be accomplished?

Of the existence of the Amyloid Substance of Bernard in the Placenta and other Organs and Tissues.

The cells of the placenta contain, during the earlier stages of embryonic life, animal dextrine, having characters identical with those of the amyloid substance of the liver: its presence may be readily demonstrated under the microscope. Bernard has discovered it in the placenta of rabbits, guinea-pigs, &c. He also made the very interesting observation that the multiple placentulae of the ruminants do not contain any amyloid substance, but that in this class of animals this substance is found in certain cells of the amnion. The presence, however, in the amnion or the placentas, of epithelial cells containing amyloid substance, is a fact quite secondary to the general fact that this substance enters largely into the constitution of most of the tissues of the embryo. Its existence does not indicate a new function of an organ doing temporarily the duty of the liver, but it indicates a new fact with regard to the development of certain structures and a new property of tissue. During embryonic life a great part of the foetal tissues are found to be so impregnated with amyloid substance, that it appears to be the formative material from which these tissues are evolved; and, in fact, it would seem to be related to their growth and development, as starch is to the growth and development of the tissues of vegetables. In the skin of the chick *in ovo*, and of the foetuses of rabbits, cats, guinea-pigs, sheep, oxen, pigs, it is readily demonstrated; it is seen by the addition of acidulated tincture of iodine, and is most abundant at the points where the aggregation of epithelial cells shows that the feathers and hairs are about being developed. The horny structures contain it plentifully; in the bill, the hoof, and the claws it exists in large proportion. From the hoof of a foetal calf of about four months enough may be obtained, by the alcoholic solution of potash, for chemical examination and fermentation. The muscular tissues of the foetus are full of it; from 20 to 50 per cent. can be extracted from the muscles of foetal calves of from three to seven months by the aid of the alcoholic solution of potash.

Having arrived, by a repetition of Dr. Pavy's ingenious experiments, at the conclusion that the amyloid substance of the liver is not normally changed into glucose, and finding on examination the accuracy of the facts concerning the physiological relations of the amyloid substance to the foetal and other tissues, discovered by M. Charles Rouget, and investigated by Bernard himself, the question presents itself, May it not be that the liver does for the adult what divers tissues do during the development of the foetus? May not this great organ form, with the help of the amyloid substance secreted in its cells, a nitrogenous compound, just as the muscles of the foetus convert the amyloid substance contained in them into the highly nitrogenous material of muscular tissue?

May not, in fact, the amyloid substance of the liver be the basis of an azotized protoplasma forming a constituent of the blood of the adult animal, as the amyloid substance of muscle is the basis of the

material from which the evolution of muscular tissue is accomplished?

Even a superficial consideration of the functions performed by the liver leads one to answer these questions in the affirmative. For if it be true that the blood which enters the liver is rich in fibrin and albumen, and that these materials are so completely changed within this organ, that little or none of them leave it by the hepatic vessels, what becomes of them? It is true their hydrocarbonous constituents may be thrown out as bile. But what of the nitrogen contained in them? If it does not escape by the bile-ducts, it has no other mode of exit save by the hepatic vessels. The author conceives it to be reunited with the hydrocarbonous amyloid substance, and to leave the liver as a newly-formed proteic compound, partly perhaps as globuline, and partly as material, in its reactions resembling caseine in some respects, in others albuminose, and which is fully described in the memoir. These considerations lead to the necessity of investigating the several distinct functions of the liver:—

- 1st. As to its action on the fibrin and albumen of the blood.
- 2nd. As to the constitution of healthy bile (so far as its azotized elements are concerned).
- 3rd. As to the relative composition and characters of the blood which enters and of that which leaves the liver.

The author adds his testimony to that of Lehmann and Brown-Séquard as regards the fibrin-destroying function of the liver; he attempts to show that, in proportion to the amount of fibrin which disappears in the liver, the quantity of nitrogen eliminated in the form of bile is very small indeed, but that the blood in passing through the liver becomes greatly enriched in colourless corpuscles, and that it contains more abundantly than other blood an azotized compound, resembling what has been described by some authors as blood-caseine. This material, although resembling, is not identical with caseine; it can be obtained from the serum of blood abstracted by a peculiarly contrived instrument from the vena cava, close to the mouths of the hepatic veins.

Whatever may be its precise chemical composition and characteristics, whether it is to be regarded as a form of albumen, or albumen-peptone (albuminose), or caseine, it is enough to state, that during active digestion the blood which leaves the liver contains a proteic compound, that it is richer in this compound than arterial blood, and that this latter is richer in it than ordinary venous blood, or than that of the portal vein. At the same time the blood of the hepatic veins contains a far larger quantity of colourless blood-corpuscles than the portal blood.

A microscopic examination of these kinds of blood shows that the colourless corpuscles are from five to ten times more numerous in the former than in the latter. Physiologists are so familiar with this fact, as well as with the chief peculiarities of the colourless corpuscles of hepatic blood, that it is unnecessary to dwell upon the circumstances which have induced some of the most distinguished among them to regard as the most important function of the liver—

the formation, or at least the rejuvenescence, of the blood-corpuscles. Dr. Carpenter conceives that the appearance of the colourless corpuscles of the blood may be regarded a phenomenon analogous to the development of cells in the albumen of seeds in the vegetable kingdom. He also supposes that these cells aid in the conversion of crude alimentary matters into proximate principles. Additional support is given to each supposition by the notion that these colourless cells stand in close relationship to the material formed in the liver, so closely resembling dextrine of vegetable origin.

It is true that there is nothing novel in the view that the liver is a great blood-forming organ, or rather that it is an organ in which certain components of the blood are disintegrated, while from some of the matter so disintegrated a constant reconstruction of the blood is going forward; yet it is certain that, not long since, physiologists would have been unwilling to admit that materials constituted as the colourless blood-cells or caseine could be formed within the liver from a substance resembling starch taking to itself nitrogen derived, as one may say, from the retrogressive metamorphosis of tissue. It is very improbable that, looking to the liver alone, such a conclusion would have been arrived at. The consideration, however, of the physiological relations of the amyloid substance (of Bernard), as regards the development of the azotized tissues of the foetus,—the fact that it is, so far as they are concerned, a protoplasma, which, by taking to itself nitrogen, terminates in the evolution of fully-formed nitrogenous tissues,—prepares one to consider the idea that the liver evolves its proteic compounds during adult life by a somewhat similar process.

To M. Charles Rouget we unquestionably owe the observation of the fundamental facts which lead to the foregoing conclusions; yet the author hopes that the recapitulation of facts in this communication will be found worthy of the consideration of physiologists; for he conceives that not only is the view of the subject which he has ventured to adopt in harmony with a great number of hitherto unexplained circumstances, but that it gives a solution more satisfactory than any yet given of certain pathological phenomena which it would be out of place to speak of here.

On the Action of the Medulla Oblongata, Spinal Cord, and Grand Sympathetic on the Bladder.

By M. GLUGE.

(*Comptes Rendus*, tome lvii. p. 565.)

The author galvanized the medulla oblongata in a young dog, after having removed the whole brain, and noticed marked contraction of the muscles of the bladder with considerable diminution in volume of the organ. No such effect was obtained by galvanizing the cerebellum, the optic thalami, corpora striata, and cerebral hemispheres. But as soon as the corpora restiformia were irritated

in that part of their course near the cerebellum, the corpora quadrigemina and cerebral peduncles, the bladder was immediately thrown into violent contraction. When one restiform body lost its irritability, the same effect was produced by galvanizing the other. Section of both pneumogastrics had no influence on the result.

In order to determine exactly the amount of contraction, the author introduced into the bladder a graduated glass tube filled with water. Spontaneous contraction of the viscera, independently of all nervous irritation, is known by its great regularity and the slight degree to which it occurs. Muscles, near the bladder, when thrown into contraction by galvanic irritation of the spinal cord, also excite contraction of the bladder, but in such cases, the effect follows immediately on applying the irritation, whereas, it is only after a minute or a minute and a half, that the organ contracts when its nerves are irritated.

By making a transverse section of the rectum, it can no longer compress the organ.

The motor nerves of the bladder are derived from two sources : the lumbar portion of the sympathetic and the third and fourth sacral nerves. But the fibres of the sympathetic distributed to the bladder do not act directly in producing contraction of the organ : their action is merely reflex. For if the trunk of the nerve be cut across near the kidneys, after its rami communicantes have been divided, galvanic irritation of its lower extremity, which is still in communication with the bladder, has no longer the property of exciting contraction of the organ. If, on the other hand, the nerve be divided on a level with the promontory of the sacrum, galvanic irritation of its upper extremity excites contraction of the bladder.

Now, with regard to the sacral nerves, the third and fourth alone were found capable of directly exciting contractions of the bladder. If they were cut across, no irritation of the medulla oblongata or of the spinal cord could make the organ contract.

The author, therefore, draws the following conclusions ;—

1. The only motor nerves of the bladder at present known are derived from the third and fourth sacral nerves.
2. The sensory nerves of the bladder communicate through the lumbar portion of the sympathetic and its rami communicantes with the spinal cord, and produce the reflex movements of the bladder.
3. Irritation of the medulla oblongata and its peduncle, and of the spinal cord, causes contraction of the bladder.

*On the Influence of the Relative Ages of the Parents upon
the Sex of the Offspring.*

By M. BOUDIN.

(*Comptes Rendus*, Mai 27, 1864.)

A careful and elaborate statistical inquiry leads M. Boudin to conclude :—1. That when the father is older than the mother, the

male sex predominates in the children. 2. That under opposite circumstances the female sex predominates. 3. That the sexes are equally divided with a slight superiority in favour of females, when the parents are of the same age. M. Boudin further remarks that other observers—viz., M. Hafacker, of Tübingen; Mr. Sadler, in England; M. Goehlert, at Vienna; and M. Boulanger, at Calais, have been led to the same conclusions by their separate inquiries.

On a Colloid Acid as a Normal Constituent of Human Urine.

By Dr. WILLIAM MARCET.

(*Proceedings of the Royal Society, June 16, 1864.*)

The object of the present communication is to describe the mode of extraction and the properties of an acid of a colloid nature which is always present in healthy human urine, and which appears destined to become of great importance in Physiological Chemistry.

With the view of separating this acid from the urinary secretion, the fluid is mixed with animal charcoal, concentrated, and filtered, and the filtrate, after precipitation with baryta-water, is dialysed for about twenty-four hours. The dialysed liquid, after subsequent filtration and concentration, is mixed with basic acetate of lead, which precipitates the colloid acid as an insoluble lead-salt, along with a little hydrochloric acid and other impurities. The precipitate should be thoroughly washed, decomposed with sulphuretted hydrogen, and again treated with animal charcoal. When the acid is required in a pure state, the hydrochloric acid present is removed with carbonate of silver, the excess of the silver precipitated with sulphuretted hydrogen, and, after boiling to evolve this last substance, basic acetate of lead is again added. The lead-salt perfectly washed may be considered pure, and the pure acid can be obtained from it by decomposition with sulphuretted hydrogen.

The acid is very slow to decompose when exposed to the air. It may be considered to undergo no loss or decomposition by being boiled, as shown by direct experiment. After concentration by heat, its colour darkens and it becomes syrupy, possessing a sharp acid taste, with a slight acrid and astringent after-taste; the taste is perceptible in the solution when very dilute: no crystals of the acid could be obtained in the syrup. Dried at a temperature under 212° F., the acid has the appearance of a transparent varnish; it is very hygroscopic, and dissolves readily in water, though not apparently in alcohol (sp. gr. '820) or in ether. When burnt, the colloid acid chars, emitting a pungent and irritating smell, and after complete combustion, nothing but the minutest trace of inorganic residue remains. Although strictly a colloid, this acid in the free state passes through a dialyser, but not so readily as a crystalloid. When under the form of a compound, its property of dialysing appears much

diminished. Dr. Marcet could not find that it exerted any action on polarized light.*

Composition of the Colloid Acid.

The acid was found to consist only of carbon, hydrogen, and oxygen. Dr. Marcet has not yet succeeded in establishing its ultimate quantitative composition, but it appears to be very poor in hydrogen and rich in carbon. The atomic weight of the substance was found by the analysis of its insoluble lead-salt, and of its baryta-salt. On determining the lead in the lead compounds from six different samples of urine, the average in 100 parts was—

Oxide of lead	66·3
Acid	33·7
<hr/>	
	100·0

The analysis of the baryta compound yielded in 100 parts—

Baryta	72·2
Acid	27·8
<hr/>	
	100·0

Corresponding to the atomic weights

for the lead compound { Oxide of lead	111·5
{ Acid	56·7
<hr/>	
	168·2
for the baryta compound { Baryta	76·5
{ Acid	29·5
<hr/>	
	106·0

It is therefore very obvious that the acid forms two salts, an acid and a neutral salt; we shall adopt the number 28·35 (or $\frac{56·7}{2}$) for the atomic weight of the new acid. The fact of there existing two different compounds of the acid, explains many chemical phenomena exhibited by this substance and its salts.

Compounds of the Colloid Acid of Urine.

The neutral salts are all soluble.

Lead-Salts.—The colloid acid forms two lead-salts—one which is insoluble in water, and contains two equivalents of acid, and one which is soluble in water, and evidently contains one equivalent of acid.

The insoluble compound is obtained by adding basic acetate of lead to an aqueous solution of the acid or of its neutral salts. An excess of the basic acetate redissolves the precipitate, which reappears on the addition of dilute nitric acid, to be finally redissolved

* This acid does not precipitate egg-albumen. It precipitates casein, but an excess does not appear to redissolve the precipitate, as in the case of acetic acid.

in an excess of the mineral acid. The whole of the colloid acid is not, however, precipitated by basic acetate of lead, principally on account of the formation of a certain quantity of neutral acetate of lead, which I found to have the property of dissolving the insoluble colloid lead-salt. On boiling a mixture of the insoluble lead compound with neutral acetate of lead, acetic acid was given off, a confirmatory proof of the insoluble lead compound being an acid salt; in this case one equivalent of the colloid acid combines with one equivalent of oxide of lead of the neutral acetate, two equivalents of the neutral lead-salt of the colloid acid being thus formed,



This shows that it is not possible to estimate with accuracy the amount of the acid in urine by means of basic acetate of lead.

When the acid is boiled with an excess of hydrated oxide of lead, an insoluble compound is formed; if the acid be in excess, a compound soluble in hot water, but precipitating on cooling, is obtained: I have not yet determined the composition of these two lead-salts.

Baryta- and Lime-Salts.—These salts are easily prepared from the carbonates. They contain one equivalent of the acid, are soluble in water, and yield precipitates with basic acetate of lead, nitrate of silver, and protonitrate of mercury and tannic acid; the more concentrated the solution, the more abundant the precipitates. A very slight precipitate occurs by adding neutral acetate of lead to salts of the acid; other reagents fail to yield precipitates.

The acid dissolves silver from the carbonate, but it could not be neutralized perfectly by such means. The lime-salt of the acid cannot be entirely decomposed by boiling it with carbonate of silver.

When the acid is boiled with black oxide of copper, copper is readily dissolved.

Physiological Relations of the Colloid Acid of Urine.

Dr. Marcket endeavoured to determine approximately the quantity of the colloid in a given bulk of the urinary secretion, and extracted from 8 litres 4.46 grammes of this substance, which, however, must fall short considerably of the real amount of the acid present.

It may be considered as existing in all probability in the blood, where there is little doubt that it acts an important part in the phenomena of the secretion of gastric juice, by displacing the hydrochloric acid from chloride of sodium, combining with the sodium; the soda-salt would remain in the blood, being a colloid compound, while the free hydrochloric acid would find its way into the stomach. An experiment performed in connexion with this subject bears out the present view.

The formation of the colloid acid appears to result from some transformation of the colloid non-nitrogenous constituent of the liver known as the glucogenic substance. When better acquainted with the chemical composition and physiological relations of the colloid acid of urine, Dr. Marcket will be able to give it an appropriate name.

Observations on the Absorbing Power of the Human Skin.

By Dr. MURRAY THOMPSON, Lecturer on Chemistry,
Edinburgh Med. School.

(*Edinburgh Medical Journal*, May, 1863.)

These observations were made chiefly with the view of testing the curative power of mineral water baths; and the conclusion to which they lead is not only that absorption by the skin has been greatly exaggerated, but that, in the case of substances in aqueous solution, it seems to be the exception and not the rule for absorption to take place.

"The experiments," says Dr. Thompson, "were all made on my own person. The general method of conducting them was this:—The baths were taken at night before going to bed, and usually after six hours' abstinence from both food and drink. The quantity of water in the bath was measured; its temperature was taken, and always maintained at the same point throughout the experiment by small additions of boiling water. The quantity of the salt to be used was weighed and dissolved. The state of the urine as to acidity and alkalinity was examined, and, of course, as to the absence of the substance about to be used in the bath. Lastly, the time of immersion was noted. The whole body, except the head and neck, was immersed. No precaution was taken to avoid inhaling the steam arising from the warm water, as no substance was employed which is volatile at temperatures beneath 100° F.

"It may be well that I should now proceed to state shortly the circumstances attendant on each experiment.

"I. March, 1860.—I took a bath containing eighty gallons of warm water (the temperature has not been recorded in this case). Dissolved in it there was half an ounce of iodide of potassium. Time in bath, 40 minutes. *Remarks.*—No urine was passed till next morning. On testing some of that which passed then, I could not find iodine. The remainder of the urine, along with all that was voided that day, was collected and evaporated to one-third its total bulk. On testing this concentrated fluid again, I could not detect iodine. The test for iodine made use of in this and the other trials consisted in using a solution of nitrate of potass mixed with dilute hydrochloric acid, instead of chlorine water. Price, who first used this modification of the old test, states that by it he could discover easily the three-millionth of a grain of iodine. Starch paste is used as well as the above mixture, as in the old test.

"II. January 12, 1861.—Nine months after the first experiment I made the second. The reason of this long interval will appear afterwards. I used about sixty gallons of water at a temperature 100° to 104°; and dissolved in it one ounce (avoirdupois) of iodide of potassium. Time in bath, 30 minutes. Urine of all the next day collected and concentrated as before. On testing for iodine, I could not detect a trace.

"III. June 27, 1861.—A bath taken, containing fifty gallons of

water at 92° , with 200 grains of iodide of potassium dissolved in it. Time in bath, 40 minutes. No towel was used after this bath: the water adhering to the skin was left to dry there. As before, the whole urine of the next day was collected and concentrated to about one-third, and then, on testing, not a trace of iodine could be found.

"IV. July 16, 1861.—A bath taken, containing forty gallons of water at 100° . There was mixed with the water 6 fluid ounces of a solution of caustic soda of sp. gr. 1052 = 4 per cent.; this made the bath-water quite alkaline to test-paper. Time in bath, 30 minutes. The urine voided before the bath was distinctly acid. The first urine voided after the bath was distinctly acid also; and so was that passed the next morning, as well as several times during that day.

"V. November 16, 1861.—Took a bath, containing about fifty gallons of water at 90° . Dissolved in it 1400 grains of ferrocyanide of potassium. Time in bath, one hour. Urine of all next day and one-half of the following was collected, and having been concentrated, it was tested for the ferrocyanide, but not a trace was found.

"VI. November 23, 1861.—Took a bath, containing seventy gallons of water at 98° , with 1400 grains of the ferrocyanide dissolved in it. Time in bath, one hour and a quarter. Urine of all next day and part of the one following was treated as before, but not a trace of the prussiate of potass could be found. In this trial part of the saliva was also collected. I could not find in it a trace of the salt used in the bath.

"In order to try whether frequency of bathing would render the skin in any way more permeable, I had a bath every night for six nights in succession—the evenings being from Monday the 2nd December 1861, till Saturday the 7th December. The following is the record of these trials:—

"VII. Seventy-two gallons of water at 96° , containing 2000 grains of ferrocyanide of potassium. Time in bath, one hour. Urine of next day, treated as before, gave no indication of containing the ferrocyanide.

"VIII. Fifty-four gallons of water at 95° had dissolved in it 5000 grains of ferrocyanide of potassium. Time in bath, one hour. Urine of next day, treated as before, showed not a trace of ferrocyanide.

"IX. Forty-four gallons of water at 95° had mixed in it 15 fluid ounces of soda solution of sp. gr. 1070 = 5·5 per cent. Time in bath, one hour. Urine tested twice during four hours preceding the bath, was found to be acid. The same reaction was also manifested several times during the next day.

"X. Forty-four gallons of water at 97° , containing 26 fluid ounces of the same solution of caustic soda as was used in IX. Time in bath, one hour. Urine before and after the bath, acid.

"XI. Forty-two gallons of water at 95° , containing 2 lbs. (avoirdupois) of common washing soda (carbonate of soda crystals). The solution in this bath was so caustic as to make the skin have, while

bathing, the soapy feeling caused by strong alkaline solutions. Time in bath, 45 minutes. The urine both before and after this bath, as well as what was passed during the whole of the next day, was slightly acid.

"XII. Thirty-six gallons of water at 93°, had dissolved in it 1320 grains of iodide of potassium. Time in bath, one hour. All the urine of next day and part of the following was collected, evaporated to one half of its whole bulk, and this fluid carefully tested for iodine, but the result was entirely negative.

"I have here recorded the results of twelve experiments. I might add to these the results of other three which were tried during the interval of nine months that elapsed between experiments I. and II. The reason why these were not entered in their proper order was, that, unfortunately, when from home I lost the note-book containing the details concerning them. My memory, however, is quite clear as to the negative result obtained in them. In no case was a trace of the salt used (iodide of potassium in two baths, and ferrocyanide in a third) found in the urine.

"In order to make the foregoing experiments still more useful as quantitative trials, as well as to compare absorption from the common mucous surface of the stomach, &c., with absorption from the skin, I tried the following:—I swallowed a grain of iodide of potassium at night (December 10, 1861), and on testing the urine next morning, and again after concentration, I could not detect iodine. I then on another night swallowed a grain and a half, and again failed to find iodine in the urine. On another night two grains of iodide were taken, and this time iodine was easily discovered in the urine next day twice or thrice, without concentration, but the reaction was not strongly given.

"Two similar experiments were tried with ferrocyanide of potassium, when I found the smallest quantity of that salt which could be swallowed and afterwards detected in the urine was five grains. Now, to connect these experiments: it would appear that, if a half-pint (10 fluid ounces) of the water in the bath (of experiment XII.), had entered the blood through the skin—not an extravagant quantity, one would think, when the whole body was immersed—we should have had amply sufficient ($2\frac{1}{2}$ grains) of the iodide in the system to have enabled us to detect it in the urine as it passed out. So, in experiment VIII., if 10 fluid ounces of the bath-water had been taken up, I should have found ferrocyanide in the urine without concentrating it; but not a trace of that salt could be found in all the urine of the next day evaporated to one-third of its whole bulk.

"As my object in undertaking these experiments has been to realize in my own case the truth of many statements which have been made as to the curative power of mineral-water baths, depending on the absorption by the skin from them of certain salts and other substances, which they hold in solution, I have, in the foregoing experiments, followed out the circumstances pursued in the ordinary practice of bathing at thermal and other mineral-water

stations. If I have varied from that, it has been in a direction more favourable to absorption.

"In the greater number of the baths, it will be seen that I did not employ a heat so high as to produce congestion of the skin, which might have been followed by diaphoresis, the very opposite process to absorption. I did not notice sweating after any of the baths.

"The quantities of the substances used in the baths were varied considerably; so as to obviate any objection that might be made on the ground that the solutions were either too weak or too strong.

"The time through which a bath lasted was never less than half an hour,—this being about the maximum period of a warm bath,—but, to favour absorption, the time was prolonged in most cases to one hour."

Experimental Inquiries respecting Cutaneous Absorption.

By M. PARISOT.

(*Comptes Rendus*, tome lvi. pp. 327 and 373.)

The most powerful argument in favour of cutaneous absorption is derived from the presence, in the liquids of the body, of saline or other substances dissolved in water used for baths, lotions, &c. The author, therefore, endeavoured to ascertain whether such was really the fact. He used substances that had no chemical action on the skin, that did not enter normally into the composition of our liquids, that were not decomposed in our tissues, and the presence of which could be easily detected in the excreta. They were: iodide of potassium, the yellow prussiate of potash, chlorate of potash, sulphate of iron, belladonna, digitalis, and rhubarb. The experiments were carried on in his own person. In some cases, iodide of potassium and chlorate of potash were dissolved in baths used by young children. Hot summer and autumn days were selected, the thermometer ranging from 18° to 27° C; the temperature of the baths was never less than 28° nor greater than 30°; and the period of immersion lasted from one to two hours for the author, and from half-an-hour to an hour for the children. Wooden baths, carefully covered over, were used in every instance. The experiments were further carried on in the morning, fasting; the urine and the saliva were examined on every occasion before the bath, and the same substance was used several days running. The saliva and the urine were tested several times during the day after each bath, and for a week following.

The conclusions which the author came to were that saline substances and colouring matters dissolved in water are never absorbed by the skin, even after immersion for two hours. No trace of them can be detected in the urine and saliva, although their presence in those secretions can be easily shown after they have been administered internally.

Aqueous solutions of vegetable poisons, such as digitalis and atropine, are not absorbed by the skin either, for prolonged immersion in baths containing these substances never gives rise to the slightest symptom of poisoning.

The anatomical structure of the skin, continues the author, is an *a priori* argument against the absorbing capabilities of its superficial layer. The sebaceous secretion of the epidermis provides it with a sort of protecting varnish against the introduction of liquids. The palm of the hand and the sole of the foot, being the only regions without sebaceous glands, are also the only portions of the skin capable of imbibition. This view was tested by immersing dead bodies of children, for a considerable number of hours, in water of variable temperature. The bodies were weighed before and after immersion. Care was taken that no abrasion of the skin existed, and that the natural apertures of the body were perfectly closed by a layer of Venice turpentine. It was found that the bodies weighed more after immersion, and that they fell down to their original weight after a time, from evaporation. The palms of the hands and the soles of the feet were soddened and wrinkled. But on covering these parts with a thick layer of Venice turpentine, no imbibition was found to occur, even after immersion prolonged for twenty-four hours. The conclusion to be drawn from this is obvious enough.

The author next tried whether substances dissolved in alcohol, ether, or chloroform, which have the property of dissolving the secretions of sebaceous glands, were absorbed by the skin. He found that atropine dissolved in chloroform produced dilatation of the pupils and a disturbance of vision within three minutes after its application on the forehead. The same quantity of atropine dissolved in alcohol produced similar effects, only in half-an-hour. If the alkaloid were dissolved in water feebly acid from the addition of acetic acid, no dilatation of the pupil ensued,

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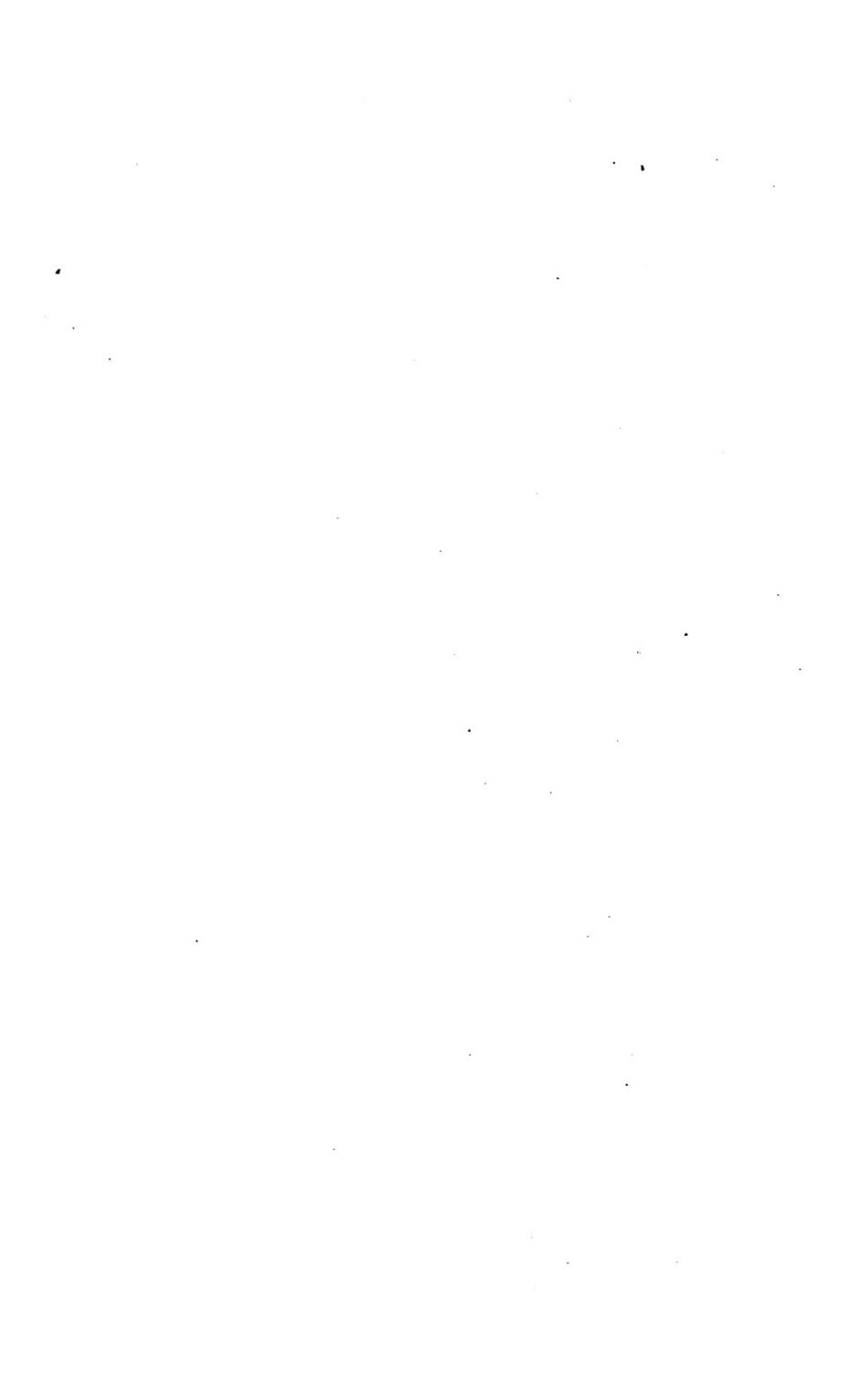
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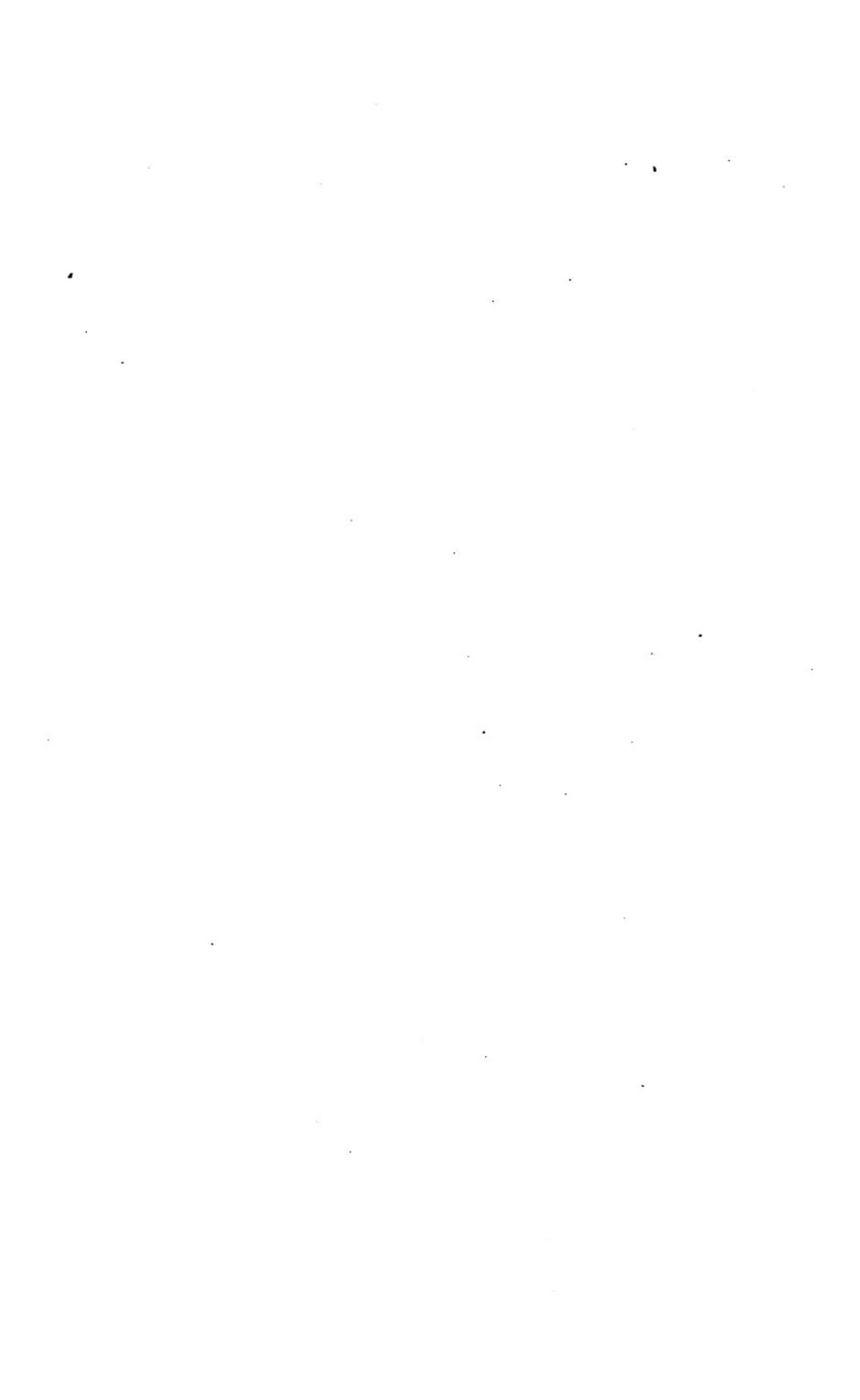
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